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Philip Earl Stanhope.



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

I N G E N I O U S,

I N M A N Y

Confiderable Parts of the W O R L D.

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PHILOSOPHICAL TRANSACTIONS.

For the Month of *January*, 1747-8.

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A Letter to the Right honourable George Earl of Macclesfield concerning an apparent Motion observed in some of the fixed Stars ; by James Bradley D. D. Astronomer Royal, and F. R. S.

My LORD,

*Read at a Meeting
of the Royal Society,
Febr. 14. 1747.*

THE great Exactness, with which Instruments are now constructed, hath enabled the Astronomers of the present Age to discover several Changes, in the Positions of the heavenly Bodies; which, by reason of their *Smallness*, had escaped the Notice of their Predecessors. And altho' the Causes of such Motions have always subsisted, yet Philosophers had not so fully consider'd, what the Effects of those known Causes would be, as to demonstrate *a priori* the *Phænomena* they might produce; so that Theory itself is here, as well as in many other Cases, indebted to Practice, for the Discovery of some of its most elegant Deductions. This points out to us the great Advantage of cultivating *this*, as well as every other Branch of Natural Knowledge, by a regular Series of Observations and Experiments.

The Progress of Astronomy indeed has always been found, to have so great a Dependence upon
A accurate

accurate Observations, that, till such were made, it advanced but slowly : For the first considerable Improvements that it received, in point of Theory, were owing to the renowned *Tycho Brakè*; who far exceeding those that had gone before him, in the Exactness of his Observations, enabled the sagacious *Kepler* to find out some of the principal Laws, relating to the Motion of the heavenly Bodies. The Invention of Telescopes and Pendulum-Clocks affording proper Means of still farther improving the *Praxis* of Astronomy; and these being also soon succeeded by the wonderful Discoveries made by our Great *Newton*, as to its Theory; the Science, in both respects, had acquired such extraordinary Advancement, that future Ages seemed to have little room left, for making any great Improvements. But, in fact, we find the Case to be very different; for, as we advance in the means of making more nice Inquiries, new Points generally offer themselves, that demand our Attention. The Subject of my present Letter to your Lordship, is a Proof of the Truth of this Remark : for, as soon as I had discovered the Cause, and settled the Laws of the Aberrations of the fixed Stars, arising from the Motion of Light, &c. whereof I gave an Account in N^o. 406. of the *Philosophical Transactions*; my Attention was again excited by another new *Phænomenon*, viz. an annual Change of Declination in some of the fixed Stars; which appeared to be sensibly greater about that time, than a Precession of the Equinoctial Points of 50th in a Year would have occasioned. The Quantity of the Difference, tho' small in itself, was

was rendered perceptible, thro' the Exactness of my Instrument, even in the first Year of my Observations; but being then at a Loss to guess, from what Cause that greater Change of Declination proceeded, I endeavoured to allow for it in my Computations, by making use of the *observed* annual Difference, as mentioned in *p. 652.* of the same *Transaction*.

From *that* time to the present, I have continued to make Observations at *Wansted*, as Opportunity offered, with a View of discovering the Laws and Cause of this *Phænomenon*: For, by the Favour of my very kind and worthy Friend *Matthew Wymondesold* Esq, my Instrument has remained, where it was first erected; so that I have been able, without any Interruption, which the Removal of it to another Place would have occasioned, to proceed on with my intended Series of Observations, for the Space of twenty Years: a Term somewhat exceeding the whole Period of the Changes, that happen in this *Phænomenon*.

When I shall mention the *small* Quantity of the Deviation, which the Stars are subject to, from the Cause that I have been so long searching after; I am apprehensive, that I may incur the Censure of some Persons, for having spent so much Time in the Pursuit of such a seeming Trifle: But the candid Lovers of Science will, I hope, make due Allowance for that natural Ardour, with which the Mind is urged on towards the Discovery of Truths, in themselves perhaps of *small* Moment, were it not that they tend to illustrate others of greater Use.

The apparent Motions of the heavenly Bodies are so complicated, and affected by such a Variety of

Causes; that in many Cases it is extremely difficult to assign to each its due Share of Influence; or distinctly to point out, what Part of the Motion is the Effect of one Cause, and what of another: And whilst the joint Effects of *All* are only attended to, great Irregularities and seeming Inconsistencies frequently occur; whereas, when we are able to allot to each particular Cause its proper Effect, Harmony and Uniformity usually ensue.

Such seeming Irregularities being also blended with the unavoidable Errors, which Astronomical Observations must be always liable to, as well from the Imperfection of our Senses, as of the Instruments that we make use of, have often very much perplex'd those, who have attempted to solve the *Phænomena*: and till Means are discovered, whereby we can separate and distinguish the *particular* Part of the whole Motion, that is owing to each respective Cause, it will be impossible, to be well assured of the Truth of any Solution. For these Reasons, we generally find, that the more exact the Instruments are, that we make use of, and the more regular the Series of Observations is, that we take; the sooner we are enabled to discover the Cause of any new *Phænomenon*. For when we can be well assured of the Limits, wherein the Errors of the Observations are contain'd; and have reduced them within as narrow Bounds as possible, by the Perfection of the Instruments which we employ; we need not hesitate to ascribe such apparent Changes, as manifestly exceed those Limits, to some other Causes. Upon these Accounts it is incumbent upon the *practical* Astronomer,

Astronomer, to set out at first with the Examination of the Correctness of his Instruments; and to be assured that they are sufficiently exact for the Use he intends to make of them: or at least he should know, within what Limits their Errors are confined.

This Practice has, in an eminent manner, been lately recommended by your Lordship's noble Example; who having, out of a singular Regard for the Science of Astronomy, erected an Observatory, and furnished it with as complete an *Apparatus* of Instruments, as our best Artists could contrive; would not fully rely on their Exactness, till their Divisions had undergone the strictest Re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human Skill can at present produce.

The Lovers of *this* Science in general, cannot but acknowledge their Obligations to your Lordship on this Account; but I find myself more particularly bound to do it; since, by means of your Lordship's most accurate Observations, I have been enabled to settle some *principal Elements*; which I could not at present otherwise have done, for want of an Instrument at the Royal Observatory, *proper* for that Purpose: For the large *mural Quadrant*, which is there fixed to observe Objects lying Southward of the Zenith, however *perfect* an Instrument it may be in it self, is not *alone* sufficient to determine, with proper Exactness, either the *Latitude* of the Observatory, or the Quantity of Refraction corresponding to different Altitudes: For it being too heavy to be conveniently removed; and the Room wherein it is placed, being too small to admit of its

being turned to the opposite Side of the Wall, whereon it now hangs; I cannot, by *actual* Observations of the circumpolar Stars, settle those necessary Points; and therefore have endeavoured to do it, by comparing my own with your Lordship's Observations: and until this Defect in the *Apparatus* belonging to the Royal Observatory be removed, we must be indebted to your Lordship, for the Knowledge of its true Situation.

A Mind intent upon the Pursuit of any kind of Knowledge, will always be agreeably entertained, with what can supply the most proper means of attaining it: Such, to the practical Astronomer, are exact and well-contriv'd Instruments; And I reflect with Pleasure on the Opportunities I have enjoyed, of cultivating an Acquaintance and Friendship with the Person, that, of all others, has most contributed to their Improvement. For I am sensible, that if my own Endeavours have, in any respect, been effectual to the Advancement of Astronomy; it has principally been owing to the Advice and Assistance given me by our worthy Member Mr. *George Graham*; whose great Skill and Judgment in Mechanics, join'd with a complete and practical Knowledge of the Uses of Astronomical Instruments, enable him to contrive and execute them in the most perfect manner.

The Gentlemen of the *Royal Academy of Sciences*, to whom we are so highly obliged for their exact Admeasurement of the Quantity of a Degree under the Arctic Circle, have already given the World very convincing Proofs of *his* Care and Abilities in those Respects; and the particular Delineation, which they have lately published, of the several Parts
of

of the *Sector*, which he made for them, hath now rendered it needless, to enter upon any minute Description of *mine* at *Wansted*; both being constructed upon the same Principles, and differing in their component Parts, chiefly on account of the different Purposes, for which they were intended.

As mine was originally designed to take only the *Differences* of the Zenith Distances of Stars, in the various Seasons of the Year, without any View of discovering their *true* Places; I had no Occasion to know exactly, what Point on the Limb corresponded to the *true* Zenith: and therefore no Provision was made in my Sector, for the changing of its Situation for that Purpose. Neither was it necessary that the Divisions or Points on the Arc should be set off, with the utmost Accuracy, Equidistant from each other; because, when I observe any particular Star, the same Spot or Point being first bisected by the Plumb-line, and then the Screw of the Micrometer turn'd until the Star appears upon the middle of the Wire, that is fixed in the common Focus of the Glasses of the Telescope; I can thereby collect, how far the Star is from that given Point at the Time of Observation: and afterwards, by comparing together the several Observations that are made of it, I am able to discover what apparent Change has happen'd. The Quantity of the visible Alteration, in the Position of the Stars, being expressed by Revolutions and Parts of a Revolution, of the Screw of the Micrometer; I endeavoured to determine, with great Care, the true Angle answering thereto: and after various Trials, I thoroughly satisfied myself, both of the Equality of

the Threads of the Screw, and of the precise Number of Seconds corresponding to them.

But altho' these Points could be settled with great Certainty, I was nevertheless obliged to make one Supposition; which perhaps to some Persons may seem of too great Moment in the present Inquiry, to be admitted without an evident Proof from Facts and Experiments. For I *suppose*, that the Line of Collimation of my Telescope has invariably preserved the same Direction, with respect to the Divisions upon the Arc, during the whole Course of my Observations. And indeed it was on account of the Objections, which might have been raised against such a *Postulate*, that I thought it necessary, to continue my Series of Observations for so many Years, before I published the Conclusions, which I shall at present endeavour to draw from them.

Whoever compares the Result of the several Trials, that have been made by the Gentlemen of the *Academy of Sciences*, for determining the Zenith Point of their Sector, since their Return from the North; will, I presume, allow that *mine* is not an unreasonable or precarious *Supposition*: since it is evident, from their Observations, that the Line of Collimation of that Instrument underwent no sensible Change in its Direction, during the Space of more than a whole Year; altho' it was several times taken down, and set up again, in different and remote Places; whereas *mine* hath always remained suspended in the same Place.

But besides such a strong Argument for the Probability of the Truth of my *Supposition*, I have the Satisfaction of finding it *actually* verified by the Observations

Observations themselves; which plainly prove, that at the End of the full Period of the Deviations which I am going to mention, the Stars are found to have the same Positions by the Instrument, as they ought to *have*, supposing the Line of Collimation to have continued unaltered from the Time when I first began to observe.

I have already taken notice, in what manner this *Phænomenon* discover'd itself to me at the End of my first Year's Observations, *viz.* by a *greater* apparent Change of Declination in the Stars near the Equinoctial Colure, than could arise from a Precession of 50'' in a Year; the mean Quantity now usually allowed by Astronomers. But there appearing at the same time, an Effect of a quite contrary Nature, in some Stars near the Solstitial Colure, which seem'd to alter their Declination *less* than a Precession of 50'' required; I was thereby convinced, that all the *Phænomena*, in the different Stars, could not be accounted for, merely by supposing, that I had assumed a wrong Quantity for the Precession of the Equinoctial Points.

At first, I had a Suspicion, that some of these small apparent Alterations in the Places of the Stars, might possibly be occasioned by a Change, in the Materials, or in the Position of the Parts of my Sector: But, upon considering how firmly the Arc, on which the Divisions or Points are made, is fastened to the Plate, wherein the Wire is fixed that lies in the Focus of the Object-Glass; I saw no Reason to apprehend, that any Change could have happened in the Position of that Wire and those Points. The Suspension therefore of the Plummets being the most likely Cause, from whence I conceived any Uncer-

tainty could arise; and the Wire of which had been broken three or four times in the first Year of my Observations: I attempted to examine, whether Part of the 'foremention'd apparent Motions might not have been owing, to the different Plumb-lines that had been made use of. In order to determine this, I adjusted a particular Point of the Arc to the Plumb-line, with all the Exactness I could; and then taking off the old Wire, I immediately hung on another, with which the same Spot was again compared. I repeated the Experiment three or four times, and thereby fully satisfied myself, that no sensible Error could arise from the Use of different Plumb-lines; since the various Adjustments of the same Point agreed with each other, within less than half a Second.

Having then, from such Trials, sufficient Reason to conclude, that these *second* unexpected Deviations of the Stars, were not owing to any Imperfection of my Instrument; after I had settled the Laws of the Aberrations arising from the Motion of Light, &c. I judged it proper to continue my Observations of the same Stars; hoping that, by a regular and longer Series of them, carried on thro' several succeeding Years, I might, at length, be enabled to discover the *real* Cause of such apparent Inconsistencies.

As I resided chiefly at *Wansted*, after my Sector was erected there in the Year 1727. till the Beginning of *May* 1732. when I removed from thence to *Oxford*: I had, during my Abode at *Wansted*, frequent Opportunities of repeating my Observations; and thereby discovered so many Particulars relating
to

to these *Phænomena*, that I began to guess what was the real Cause of them.

It appeared from my Observations, that, during this Interval of Time, some of the Stars near the Solstitial Colure, had changed their Declinations $9''$ or $10''$ *less*, than a Precession of $50''$ would have produced; and, at the same time, that, others near the Equinoctial Colure, had altered theirs about the same Quantity *more*, than a like Precession would have occasioned: the North Pole of the Equator seeming to have approached the Stars, which come to the Meridian with the Sun, about the Vernal Equinox and the Winter Solstice; and to have receded from those, which come to the Meridian with the Sun, about the Autumnal Equinox and the Summer Solstice.

When I consider'd these Circumstances, and the Situation of the Ascending Node of the Moon's Orbit, at the time when I first began my Observations; I suspected, that the Moon's Action upon the Equatorial Parts of the Earth might produce these Effects: For, if the Precession of the Equinox be, according to Sir *Isaac Newton's* Principles, caused by the Actions of the Sun and Moon upon those Parts; the Plane of the Moon's Orbit being at *one* time, above ten Degrees *more* inclined to the Plane of the Equator, than at *another*; it was reasonable to conclude, that the Part of the whole annual Precession, which arises from her Action, would in different Years be varied in its Quantity; whereas the Plane of the Ecliptic, wherein the Sun appears, keeping always nearly the same Inclination to the Equator; *that* Part of the Precession, which is owing to the Sun's Action, may be the same every

Year: And from hence it would follow, that, altho' the *mean* annual Precession, proceeding from the joint Actions of the Sun and Moon, were 50''; yet the *apparent* annual Precession might sometimes exceed, and sometimes fall short, of that mean Quantity, according to the various Situations of the Nodes of the Moon's Orbit.

In the Year 1727. when my Instrument was first set up, the Moon's Ascending Node was near the Beginning of *Aries*; and consequently, her Orbit was as much inclined to the Equator, as it can at any time be; and then the *apparent* annual Precession was found, by my first Year's Observations, to be greater than the *mean*: which proved, that the Stars near the Equinoctial Colure, whose Declinations are most of all affected by the Precession, had changed *theirs*, above a tenth Part more than a Precession of 50'' would have caused. The succeeding Years Observations proved the same Thing; and in three or four Years time the Difference became so considerable, as to leave no Room to suspect, that it was owing to any Imperfection, either of the Instrument or Observations.

But some of the Stars, which I had observed, that were near the Solstitial Colure, having appeared to move, during the same time, in a manner contrary to what they ought to have done, by an Increase in the Precession; and the Deviations in them being as remarkable as in the others, I perceived that something more, than a mere Change in the Quantity of the Precession, would be requisite to solve this Part of the *Phænomenon*. Upon comparing my Observations of Stars near the Solstitial Colure, that were
almost

almost opposite to each other in Right Ascension, I found, that they were equally affected by this Cause; for whilst γ *Draconis* appeared to have moved Northward, the small Star, which is the 35th *Camelopardali Hevel.* in the *British* Catalogue, seem'd to have gone as much towards the South: which shew'd, that this apparent Motion, in both those Stars, might proceed from a Nutation in the Earth's Axis; whereas the Comparifon of my Observations of the same Stars, *formerly* enabled me to draw a different Conclusion, with respect to the Cause of the annual Aberrations arising from the Motion of Light. For the apparent Alteration in γ *Draconis*, from *that* Cause, being as great again as in the other small Star, proved, that *that Phenomenon* did not proceed from a *Nutation* of the Earth's Axis; as, on the contrary, *this* may. Upon making the like Comparifon between the Observations of other Stars, that lie nearly opposite in Right Ascension, whatever their Situations were with respect to the Cardinal Points of the Equator, it appeared, that their Change of Declination was nearly equal, but contrary; and such a Nutation or Motion of the Earth's Axis would effect.

The Moon's Ascending Node being got back towards the Beginning of *Capricorn* in the Year 1732. the Stars near the Equinoctial Colure appeared, about that time, to change their Declinations no more, than a Precession of 50'' required; whilst some of those near the Solstitial Colure altered *theirs* above 2'' in a Year less, than they ought. Soon after, I perceived the annual Change of Declination of the former to be diminished, so as to become *less* than
50''

50'' of Precession would cause; and it continued to diminish till the Year 1736. when the Moon's Ascending Node was about the Beginning of *Libra*, and her Orbit had the *least* Inclination to the Equator. But by this time, some of the Stars near the Solstitial Colure had altered their Declinations 18'' *less*, since the Year 1727. than they ought to have done from a Precession of 50''. For γ *Draconis*, which in those nine Years should have gone about 8'' more *Southerly*, was observed in 1736. to appear 10'' more *Northerly*, than it did in the Year 1727.

As this Appearance in γ *Draconis*, indicated a Diminution of the Inclination of the Earth's Axis to the Plane of the Ecliptic; and as several Astronomers have supposed *that* Inclination to diminish regularly; if this *Phænomenon* depended upon such a Cause, and amounted to 18'' in nine Years, the Obliquity of the Ecliptic would, at that rate, alter a whole Minute in thirty Years; which is much faster than any Observations, *before* made, would allow. I had Reason therefore to think, that *some Part* of this Motion at the least, if not the *Whole*, was owing to the Moon's Action upon the Equatorial Parts of the Earth; which I conceived, might cause a libratory Motion of the Earth's Axis. But as I was unable to judge, from only nine Years Observations, whether the Axis would entirely recover the same Position, that it had in the Year 1727. I found it necessary to continue my Observations thro' a whole Period of the Moon's Nodes; at the End of which I had the Satisfaction to see, that the Stars returned into the same Positions again; as if there had been no Alteration at all in the Inclination of the Earth's Axis:
which

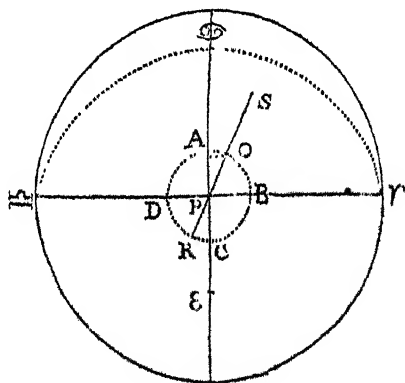
which fully convinced me, that I had guessed rightly as to the Cause of the *Phænomena*. This Circumstance proves likewise, that if there be a gradual Diminution of the Obliquity of the Ecliptic; it does not arise only from an Alteration in the Position of the Earth's Axis, but rather from some Change in the Plane of the Ecliptic itself: because the Stars, at the End of the Period of the Moon's Nodes, appeared in the same Places, with respect to the Equator, as they ought to have done, if the Earth's Axis had retained the same Inclination to an invariable Plane.

During the Course of my Observations, our ingenious Secretary of the *Royal Society*, Mr. *John Machin*, being employed in considering the Theory of Gravity; and its Consequences, with regard to the Celestial Motions; I acquainted him with the *Phænomena* that I had observed: and at the same time mentioned, *what* I suspected to be the Cause of them. He soon after sent me a Table, containing the Quantity of the annual Precession in the various Positions of the Moon's Nodes, as also the corresponding Nutations of the Earth's Axis; which was computed upon the *Supposition*, that the *mean* annual Precession is $50''$, and that the Whole is governed by the Pole of the Moon's Orbit only: and therefore he imagined, that the Numbers in the Table would be too *large*; as in Fact they were found to be. But it appeared, that the Changes which I had observed, both in the annual Precession and Nutation, kept the same Law, as to increasing and decreasing, with the Numbers of his Table. Those were calculated upon the *Supposition*,
that

that the Pole of the Equator, during a Period of the Moon's Nodes, moved round in the Periphery of a little Circle, whose Center was $23^{\circ} 29'$ distant from the Pole of the Ecliptic; having itself also an angular Motion of $50''$ in a Year, about the same Pole: The North Pole of the Equator was conceived to be in *that* Part of the small Circle, which is farthest from the North Pole of the Ecliptic, at the Time when the Moon's Ascending Node is in the Beginning of *Aries*: and in the opposite Point of it, when the same Node is in *Libra*.

Such a Hypothesis will account for an Acceleration and Retardation of the annual Precession; as also for a Nutation of the Earth's Axis: And if the Diameter of the little Circle be supposed equal to $18''$; which is the whole Quantity of the Nutation, as collected from my Observations of γ *Draconis*: then all the *Phænomena* in the several Stars which I observed, will be very nearly solved by it.

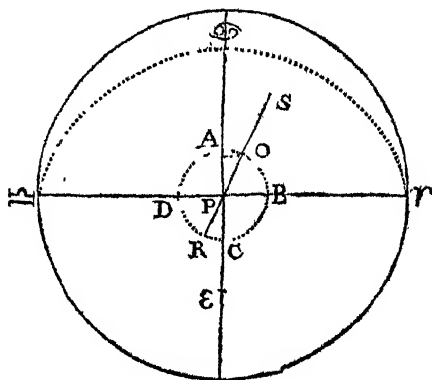
Let *P* represent the mean Place of the Pole of the Equator, about which Point, as a Center, suppose the true Pole to move in the Circle *ABCD*, whose Diameter is $18''$. Let *E* be the Pole of the Ecliptic, and *EP* be equal to the mean Distance between the Poles of the



the Equator and Ecliptic; and suppose the true Pole

Pole of the Equator to be at *A*, when the Moon's Ascending Node is in the Beginning of *Aries*; and at *B*, when the Node gets back to *Capricorn*; and at *C*, when the same Node is in *Libra*: at which time the North Pole of the Equator being nearer the North Pole of the Ecliptic, by the whole Diameter of the little Circle *AC* equal to $18''$; the Obliquity of the Ecliptic will then be so much *less* than it was, when the Moon's Ascending Node was in *Aries*. The Point *P* is supposed to move round *E*, with an equal retrograde Motion, answerable to the mean Precession arising from the joint Actions of the Sun and Moon: while the true Pole of the Equator moves round *P*, in the Circumference *ABCD*, with a retrograde Motion likewise, in a Period of the Moon's Nodes, or of eighteen Years, and seven Months. By this means, when the Moon's Ascending Node is in *Aries*, and the true Pole of the Equator at *A*, is moving from *A* towards *B*: it will approach the Stars, that come to the Meridian with the Sun about the Vernal Equinox; and recede from those that come with the Sun near the Autumnal Equinox, *faster* than the *mean* Pole *P* does. So that, while the Moon's Node goes back from *Aries* to *Capricorn*, the *apparent* Precession will seem so much *greater* than the *mean*; as to cause the Stars, that lie in the Equinoctial Colure, to have altered their Declination $9''$, in about four Years and eight Months, *more* than the mean Precession would do: and in the same time, the North Pole of the Equator will seem to have approached the Stars, that come to the Meridian with the Sun at our Winter Solstice, about $9''$; and to have receded as *much* from those, that come with the Sun at the Summer Solstice.

Thus the *Phænomena* before recited are in general conformable to this Hypothesis. But to be more particular; let *S* be the Place of a Star, *PS* the Circle of Declination passing thro' it, representing its Distance from the mean Pole, and γ *PS* its mean Right Ascension. Then if *O* and *R* be the Points, where the Circle of Declination cuts the little Circle *ABCD*; the *true* Pole will be nearest that Star at *O*, and farthest from it at *R*; the whole Difference amounting to $18''$, or to the Diameter of the little Circle. As the true Pole of the Equator is supposed to be at *A*, when the Moon's Ascending Node is in *Aries*; and at *B*, when that Node gets back to *Capricorn*; and the angular Motion of the true Pole about *P*, is likewise supposed equal to that of the Moon's Node about *E*, or the Pole of the Ecliptic: since, in these Cases, the true Pole of the Equator is 90 Degrees before the Moon's Ascending Node, it must be so in all others.



When the true Pole is at *A*, it will be at the same Distance from the Stars that lie in the Equinoctial Colure, as the mean Pole *P* is; for I neglect at present the Case of such Stars as are *very* near the Pole of the Equator; and as the true Pole recedes back from *A* towards *B*, it will approach the Stars, that lie in that Part of the Colure represented by *Pr*; and recede from those, that lie in *PN*; not indeed

indeed with an *equable* Motion; but in the *Ratio* of the *Sine* of the Distance of the Moon's Node from the Beginning of *Aries*. For if the Node be supposed to have gone backwards from *Aries* 30° , or to the Beginning of *Pisces*; the Point, which represents the Place of the true Pole, will in the mean time, have moved in the little Circle, thro' an Arc, as *AO*, of 30° likewise: and would therefore in Effect have approached the Stars that lie in the Equinoctial Colure *PV*, and have receded from those that lie in *P*, $\approx 4^{\frac{1}{2}}$; which is the *Sine* of 30° to the *Radius AP*. For if a Perpendicular fall from *O* upon *PA*, it may be conceived as Part of a great Circle, passing thro' the true Pole and any Star lying in the Equinoctial Colure. Now the same Proportion, that holds in these Stars, will obtain likewise in all others; and from hence we may collect a general Rule, for finding how much nearer or farther, any particular Star is, to or from, the *mean* Pole, in any given Position of the Moon's Node.

For, if from the *Right-Ascension* of the Star, we *subtract* the *Distance* of the Moon's *Ascending Node* from *Aries*; then the *Radius* will be to the *Sine* of the *Remainder*, as $9''$, is to the *Number of Seconds*, that the Star is nearer to, or farther from the *True*, than the *Mean Pole*. When that *Remainder* is less than 180° , the Star is *nearer* to the *True*, than to the *Mean Pole*; and the contrary, when it is *greater* than 180° .

This Motion of the *true* Pole, about the *mean* at *P*, will also produce a Change in the *Right Ascensions* of the Stars, and in the Places of the *Equinoctial Points*; as well as in the *Obliquity* of the *Ecliptic*:

tic: and the Quantity of the Equations, in either of these Cases, may be easily computed for any given Position of the Moon's Nodes. But as it may be needless, to dwell longer on the Explication of the Hypothesis; I shall now proceed to shew its Correspondency with the *Phænomena*, relating to the Alterations of the Polar Distances of some of the Stars which I have observed: by laying before your Lordship the Observations themselves, together with the Computations that are necessary; in order to form a right Judgment about the Cause of these Appearances.

I have endeavoured to find the exact Quantity of the *mean* Precession of the Equinoctial Points, by comparing my own Observations made at *Greenwich*, with those of *Tycho Brabè* and others, which I judged to be most proper for that Purpose. But as many of the Stars, which I compared, gave a different Quantity; I shall assume the mean Result; which gives a Precession of one Degree in seventy-one Years and an half: this agreeing very well likewise with my Observations that were taken at *Wansted*. The Numbers in the following Tables, which express the Change of Declination in each Star, are computed upon the Supposition, that the *mean* Obliquity of the Ecliptic was $23^{\circ}. 28'. 30''$, and that it continued the *same*, during the whole Course of my Observations. And as the Moon's Ascending Node was in the Beginning of *Aries* about the 27th Day of *March* 1727, I have reduced the Place of each Star to *that* Time; by allowing the proper Change of Declination from that Day, to the Day of each respective Observation.

It being also necessary to make an Allowance for the *Aberrations* of Light; I have again examined
my

my Observations, that were most proper to determine the Transverse Axis of the Ellipsis, which each Star seems to describe; and have found it to be nearest to $40''$; which Number I therefore make use of in the following Computations.

The Divisions or Points upon the Limb of my Sector are placed five Minutes of a Degree from each other; and are numbered so, as to shew the Polar Distances nearly; the *true* Polar Distance exceeding that, which is shewn by the Instrument, about $1'. 35''$. When I first began to observe, I generally made use of *that* Point on the Limb, which was nearest to the Star's Polar Distance, without regarding whether it was more Northerly, or more Southerly than the Star: but as it sometimes happened, that the Original Point, with which I at first compared the Star, became, in Process of Time, pretty remote from it; I afterwards brought the Plummet to another Point, that was nearer to it; and carefully examined, what Number of Revolutions of the Screw of the Micro-meter &c. corresponded to the Distance between the different Points, that I had made use of: by which means I was able to reduce all the Observations of the same Star to the same Point, without supposing the several Divisions to be accurately $5'$ asunder.

I have expressed the Distance of each Star from the Point of the Arc, with which it was compared, in *Seconds* of a Degree and *tenth Parts* of a Second, exactly as it was collected from the Observations; altho' I am sensible, that the Observations themselves are liable to an Error of more than a *whole* Second; because I meet with some, that have been made within two or three Days of each other, that differ $2''$, even when they are not marked as *defective* in any respect. It

It would be too tedious, to set down the whole Number of the Observations that I have made; and therefore I shall give only enough of them, to shew their Correspondency with the 'forementioned Hypothesis in the several Years, wherein any were made of the Stars here recited. When *several* Observations have been taken of the same Star, within a few Days of each other, I have either set down the mean Result, or *that* Observation which best agreed with it. I have likewise commonly chosen those, that were made near the same Season of the Year, in such Stars as gave me the Opportunity of making that Choice; particularly in γ *Draconis*, which was generally observed about the End of *August* or the Beginning of *September*; *That* being the usual Time, when I went to *Wansted* on purpose to observe both *that*, and also some of the Stars in the *great Bear*. But the Weather proving cloudy, at that Season in the Year 1744, prevented my making a single Observation, either of γ *Draconis*, or any other Star, while I was there; which is the Cause of one Vacancy in a Series of 20 succeeding Years, wherein that particular Star had been observed. Such Stars, as were either not visible in the Day-time, towards the Beginning of *September*, or came at such Hours of the Night, as would have incommoded the Family of the House wherein the Instrument is fixed, were but seldom observed, after I went to reside at *Oxford*; which is the Reason, why the Series of Observations of *these* is so imperfect, as sometimes to leave a Chasm for several Years together. But notwithstanding this, I doubt not, but upon the whole they will be found sufficient, to
satisfy

satisfie your Lordship of the general Correspondency between the *Hypothesis* and the *Phænomena*, in the several Stars; how ever different their Situations are, with respect to the Cardinal Points of the Equator.

As I made more Observations of γ *Draconis* than of any other Star; and it being likewise very near the Zenith of *Wansted*; I will begin with the Recital of some of them. The Point upon the Limb, with which this Star was compared, was $38^{\circ}. 25'$ from the North Pole of the Equator, according to the Numbers of the Arc of my *Sector*. The first Column, in the following Table, shews the Year and the Day of the Month, when the Observations were made; the next gives the Number of *Seconds*, that the Star was found to be *South* of $38^{\circ}. 25'$: the third contains the Alterations of the Polar Distance, which the *mean* Precession, at the rate of one Degree in $71\frac{1}{2}$ Years, would cause in this Star, from the 27th Day of *March* 1727, to the Day on which the Observation was taken: the fourth shews the Aberrations of Light: the fifth, the Equations arising from the forementioned Hypothesis: and the sixth gives the *mean* Distance of the Star from the Point with which it was compared, found, by collecting the several Numbers, according to their Signs, in the 3d, 4th, and 5th Columns, and applying them to the *Observed Distances* contain'd in the Second.

If the Observations had been perfectly exact, and the several Equations of their *due* Quantity; then all the Numbers in the last Column would have been equal; but since they differ a little from one another; if the *mean* of All be taken, and the Extremes are

pared with it, we shall find no greater Difference, than what may be supposed to arise from the Uncertainty of the Observations themselves; it no where amounting to more than $1''\frac{1}{2}$. The Hypothesis therefore seems, in this Star, to agree extremely well with the Observations here set down; but as I had made above 300 of it; I took the Trouble of comparing each of them with the Hypothesis: and altho' it might have been expected, that, in so large a Number, some great Errors would have occurred; yet there are very few, *viz.* only eleven, that differ from the mean of these so much as $2''$; and not one that differs so much as $3''$. This surprising Agreement, therefore, in so long a Series of Observations, taken in all the various Seasons of the Year, as well as in the different Positions of the Moon's Nodes, seems to be a sufficient Proof of the Truth, both of *this* Hypothesis, and also of *that* which I formerly advanced, relating to the Aberrations of Light; since the Polar Distance in this Star may differ, in certain Circumstances, almost a Minute, *viz.* $56''\frac{1}{2}$, if the Corrections resulting from both these Hypotheses are neglected; whereas, when those Equations are rightly applied, the mean Place of the Star comes out the same, as nearly, as can be reasonably expected.

γ <i>Draconis</i>		South of 0 / 38. 25	Preces- sion.	Aberra- tion.	Nuta- tion	Mean Dist
		"	"	"	"	"
1727	September	3 70.5	— 0.4	+ 19.2	— 8.9	80.4
1728	March	18 108.7	— 0.8	— 19.0	— 8.6	80.3
	September	6 70.2	— 1.2	+ 19.3	— 8.1	80.2
1729	March	6 108.3	— 1.6	— 19.3	— 7.4	80.0
	September	8 69.4	— 2.1	+ 19.3	— 6.9	80.2
1730	September	8 68.0	— 2.9	+ 19.3	— 3.4	80.5
1731	September	8 66.0	— 3.8	+ 19.3	— 1.0	80.5
1732	September	6 64.3	— 4.6	+ 19.3	+ 2.0	81.0
1733	August	29 60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734	August	11 62.3	— 6.2	+ 16.9	+ 6.9	79.9
1735	September	10 60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736	September	9 59.3	— 8.0	+ 19.3	+ 9.0	79.6
1737	September	6 60.8	— 8.8	+ 19.3	+ 8.5	79.8
1738	September	13 62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739	September	2 66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740	September	5 70.8	— 11.3	+ 19.3	+ 1.9	80.7
1741	September	2 75.4	— 12.1	+ 19.2	— 1.1	81.4
1742	September	5 76.7	— 12.9	+ 19.3	— 4.0	79.1
1743	September	2 81.6	— 13.7	+ 19.1	— 6.4	80.6
1745	September	3 86.3	— 15.4	+ 19.2	— 8.9	81.2
1746	September	17 86.5	— 16.2	+ 19.2	— 8.7	80.8
1747	September	2 86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 Observations of β *Draconis*; which I find correspond as well with the Hypothesis, as those of γ ; but since the Positions of both these Stars, in respect to the Solstitial Colure, differ but little from each other; it will be needless to set down the Observations of β . I shall therefore proceed to lay before your Lordship, some Observations of a small Star, that is almost opposite to γ *Draconis*.

conis in Right Ascension, being the 35th *Camelopardali Hevel.* in the *British Catalogue*. Mr. *Flamsteed*, indeed, has not given the Right Ascension of this Star; but *that* being necessary to be known, in order to compute the Change of its Declination arising from the Precession of the Equinox; I compared the Time of its Transit over the Meridian, with that of some other Stars near the same Parallel; whereby I found, that its Right Ascension was $85^{\circ}. 54.\frac{1}{2}$ at the Beginning of the Year 1737.

This small Star was compared with the same Point of the Limb of my Sector, as γ *Draconis*; and the second Column, in the following Table, shews how many *Seconds* it was found to be South of that Point, at the time of each respective Observation. The other Columns contain, as in the foregoing Table, the Equations that are necessary to find, what its *mean* Distance from the same Point would have been on the 27th Day of *March* 1727, which is exhibited in the last Column. The whole Number of my Observations of this Star did not much exceed forty; the greatest Part of which were made before the Year 1730; in some of the following Years none were taken; and only a single one in any other, except in 1739. However, their Correspondency seems sufficient to evince the Truth of the Hypothesis: for if the Mean of these, contain'd in the Table, be taken, not one, among the rest of the Observations, will differ from it more than 2".

35 th <i>Camelopard.</i> <i>Hevelii.</i>		South of 0°	Precel- sion.	Aberra- tion.	Nutation.	Mean Dist South.
		38 25				
1727	October 20	73.6	+ 0.9	— 6.7	+ 8.9	76.7
1728	January 12	60.8	1.2	+ 6.1	8.8	76.9
	March 1	57.8	1.4	+ 9.4	8.7	77.3
	September 26	75.2	2.3	— 8.8	8.1	76.8
1729	February 26	56.4	2.8	+ 9.4	7.6	76.2
1730	March 3	57.8	4.4	9.4	5.4	77.0
1731	February 5	59.1	5.6	8.5	+ 3.0	76.2
1733	January 31	64.1	8.7	8.2	— 2.9	78.1
1738	December 30	61.8	17.2	4.3	6.5	76.8
1739	February 4	56.9	17.3	8.5	6.3	76.4
1740	January 20	56.0	18.6	7.0	— 4.0	77.6
1747	February 27	32.3	28.5	9.4	+ 8.4	78.6

The Observations of the foregoing Stars are the most proper, to prove the Change of the Inclination of the Earth's Axis to the Plane of the Ecliptic; those, which follow, will shew in what manner the Stars, that lie near the Equinoctial Colure, are affected, as well as others, that are differently situated, with respect to the Cardinal Points of the Equator. Some of these Stars are indeed more remote from the Zenith, than I would have chosen, if there had been others, of equal Lustre, in more proper Positions; because Experience has long since taught me, that the Observations of such Stars, as lie near the Zenith, do generally agree best with one another, and are therefore the fittest to prove the Truth of any Hypothesis. I shall begin with those near the Vernal Equinox. *α Cassiopeæ* was compared with the Point marked $34^{\circ} 55'$; and at first

was

was found to be more *Southerly*, but afterwards became more *Northerly* than that Point, as in the following Table; the last Column of which shews its mean Distance *South* of that Point on the 27th of *March* 1727. The Observation of the 23d Day of *December*, in the Year 1738, differs 3'' from the *mean* of the others; as does also another, that was taken five Days after this; neither of which being marked as uncertain, I judged it proper to insert one of them; 'altho' they give the mean Place of the Star near 2 Seconds more *Northerly* than any other, in a Series of above 100; *all* of which correspond, with the *mean* of these here recited, within less than 2''; excepting *two*, that give the Stars mean Distance almost 3'' more *Southerly*; but these last mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with several others, that were made near the same time, from which they differ almost 2''.

α <i>Cassiopeæ</i> .	South of 34. 55	Precel- sion.	Aberra- tion.	Nuta- tion.	Mean Dist. South
	" "	" "	" "	" "	" "
1727 September 9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September 17	30.8	29.4	+ 4.6	5.2	70.0
1729 June 8	35.7	43.8	— 16.3	6.8	70.0
December 3	N. 9.4	53.5	+ 16.5	7.7	68.2
1730 June 11	S. 13.8	64.0	— 16.2	8.4	70.0
December 9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732 January 8	N. 49.2	95.4	12.9	8.9	68.0
1733 January 21	64.8	116.0	+ 10.0	7.9	69.1
1734 June 13	62.8	143.8	— 10.1	5.0	69.9
December 11	105.4	153.7	+ 16.2	+ 3.7	68.2
1738 December 23	176.3	234.0	+ 15.2	— 7.2	65.7
1740 June 2	169.1	262.8	— 16.5	— 8.9	68.3
1747 February 27	332.3	397.0	+ 0.2	+ 4.7	69.6

Altho'

Altho' I have taken no Observation of τ *Persei* since the 22d Day of *January* 1740; yet, as this Star is very near the Zenith, and a sufficient Number were made about the Times when the Equation, resulting from the Hypothesis, was at its *Maximum*; I judged it proper to insert some of them in the next Table; the last Column of which shews, how much the Star's *mean* Distance was *South* of 38° . $20'$. on the 27th Day of *March* 1727. Among near 60 Observations I meet with two only, that differ from the mean of these so much as $2''$; and those differ almost as much from the mean of others, that were taken near the same time: so that the Hypothesis seems to correspond, in general, with the Observations of this Star as well, as with either of the foregoing.

τ <i>Persei</i> .	South of 38° .	ot ' 20	Precef- sion.	Aberra- ration.	Nuta- tion.	Mean Dist. South.
	"	"	"	"	"	"
1727 September 16	60.1	+	7.4	- 3.2	+ 6.7	71.6
December 29	39.7		11.9	+ 12.9	7.2	71.7
1728 December 21	22.5		27.2	12.8	8.7	71.2
1729 December 23	9.2		42.0	11.5	9.0	71.7
1731 January 3	N. 8.2		59.0	12.8	8.3	71.9
1732 January 8	22.0		74.8	12.7	6.7	72.2
1733 January 21	34.6		91.0	11.7	+ 4.3	72.4
1738 December 23	117.0		183.4	12.8	- 9.0	70.2
1740 January 22	132.5		200.2	11.7	8.6	70.8

After the last recited Observations, it may perhaps seem needless to add those of α *Persei*, which is farther from the Zenith; but however, as this Star lies very nearly at an equal Distance from the Equinoctial

Equinoctial and Solstitial Colures, and the Series of Observations of it is somewhat more complete, than that of α *Persei*; I shall insert one at least, for each Year wherein it has been observed; whereby it may appear, that the Hypothesis solves the *Phænomena* of Stars in this Situation, as exactly as in others: for if a *mean* be taken of the Numbers in the last Column of the following Table, which expresses the *mean* Distance of the Star *South* of $41^{\circ}. 5'$. on *March* 27th 1727, it will agree within two Seconds, with every one of 80 Observations, that have been made of this Star.

α <i>Persei</i>	South of ° 41. 5	Precel- tion.	Aberra- ration.	Nutation	Mean Dist South
	"	"	"	"	"
1727 December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728 April 7	87.5	14.3	— 0.8	8.2	109.2
July 5	94.6	17.7	— 11.4	8.5	109.4
December 13	65.7	23.8	+ 10.6	8.8	108.9
1729 December 3	53.4	37.2	9.7	8.9	109.2
1731 January 3	38.6	52.3	11.4	7.8	110.1
1732 January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July 11	S. 21.3	101.0	— 11.4	— 1.1	109.8
1738 December 24	N. 50.3	162.6	+ 11.2	9.0	108.5
1740 January 21	71.8	177.4	10.9	— 8.2	108.3
1747 February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given Examples of Stars, lying near both the Solstices and the *Vernal* Equinox; I shall now add the Observations of *one*, that is not far from the *Autumnal* Equinox, *viz.* α *Ursæ Majoris*, the brightest Star in that Part of the Heavens, which approaches the Zenith of *Wansted* within a Degree; and

and which, by reason of its Lustre and Position, gave me the Opportunity of making my Series of Observations of *It*, more complete than of many others. This Star was compared with the Point marked $39^{\circ}. 15'$. and was *South* of it as in the following Table; wherein your Lordship will see, that the Observations of the Years 1740 and 1741 give the Polar Distances $3''$ greater, than the *mean* of the other Years. Had there been only a single Observation taken in either of those Years, Part of this apparent Difference might have been supposed to arise from their Uncertainty; but as there were 8 Observations taken within a Week, either before or after the 3d Day of *June* 1740, which agree well with each other; and three were made within 20 Days in *September* 1741, which likewise corresponded with each other; I am inclined to think, that the 'foremention'd Differences must be owing to something else, besides the Error of the Observations. This *Phænomenon* therefore may deserve the Consideration of those Gentlemen, who have employed their Time in making Computations relating to the Quantity of the Effects, which the Power of Gravity may, on various Occasions, produce. For I suspect, that the Position of the Moon's Apogee, as well as of her Nodes, has some Relation to the apparent Motions of the Stars that I am now speaking of.

My Series of Observations of several Stars abound, of late Years, with so many and long Interruptions; that I cannot pretend to *determine* this Point; but probably the Differences before taken notice of in the Observations of α *Cassiopeæ*, and some others

that I have found likewise among the Observations of *other* Stars, that are not here recited, may be owing to such a Cause; which, altho' it should not have any large Share of Influence, may yet, in certain Circumstances, discover a Defect in a Hypothesis, that pays no Regard at all to *It*. But whether these Differences do arise from the Cause already hinted at; or whether they proceed from any Defect of the Hypothesis itself in any other respect; it will not be very *material* in point of Practice; since *that* Hypothesis, as it was before laid down, appears to be sufficient to solve all the *Phænomena*, to as great a Degree of Exactness, as we can in general *hope* or *expect* to make Observations. For if I take the *mean* of, all the Numbers in the last Column of the following Table for *Ursæ Majoris*, and compare it with any one of 164 Observations that were taken of it, the Difference will not exceed three Seconds.

<i>Ursæ Majoris</i>		South of ° ' "	Precession.	Aberration.	Nutation	Mean Dist. South.	
		39. 15					
		"	"	"	"	"	
1727	October	17	153.3	— 10.2	+ 1.0	— 5.2	138.9
1728	January	24	176.4	15.2	— 17.6	5.8	137.8
	July	17	150.8	23.9	+ 17.8	6.9	137.8
	October	11	170.6	28.2	+ 2.6	7.3	137.7
1729	January	16	196.6	33.1	— 17.8	7.8	137.9
	July	21	170.4	42.4	+ 17.8	8.4	137.4
1730	July	19	189.6	60.6	+ 17.8	9.0	137.8
	December	28	232.4	68.7	— 16.7	8.9	138.1
1731	September	18	218.1	81.9	+ 9.4	8.4	137.2
1732	January	10	250.7	87.7	— 17.7	8.0	137.3
	April	13	238.7	92.3	— 0.8	7.7	137.9
1734	J ly	11	255.7	133.3	+ 17.6	— 2.3	137.7

<i>n</i> <i>Ursæ Majoris</i>	South of ° '	Preces- sion.	Aberra- tion.	Nutation	Mean Dist South
	39 15	"	"	"	"
1735 September 10	280 8	154 6	+ 11 4	+ 1.2	138 8
1736 September 8	294 7	172 8	11 6	4.1	137 6
1737 July 3	303 0	187 8	17 2	6 1	138 5
1738 June 29	319 0	205 8	16 8	7 9	137 9
1739 April 25	348 0	220 8	2 5	8 8	138 5
1740 June 3	360 3	241 1	12 8	8 9	140 9
1741 September 23	390 9	265 0	7 9	+ 7 4	141 2
1745 September 5	466 7	337 1	12 4	- 3 3	138 7
1746 September 20	492 0	356 2	8 8	5 9	138 7
1747 September 2	507 2	373 5	13 2	7 8	139 1

You may perceive, my Lord, by inspecting the Tables which contain the Observations of *α Cassiopeæ* and *n Ursæ Majoris*; that the greatest Differences that occur therein may be diminished, by supposing the *true* Pole of the Equator to move round the Point *P*, in an *Ellipsis*, instead of a Circle. For if the transverse Axis, lying in the Direction *AC*, be 18", and the Conjugate, as *DB*, be about 16"; the Equations, resulting from such an Hypothesis, will make the Numbers in the last Columns agree with each other; nearer than as they now stand. But since this would not entirely remove the Inequalities, in all the Positions of the Moon's Nodes; I shall refer the more accurate Determination of the *Locus* of the *true* Pole to Theory; and at present only give the Equations for the Precession of the Equinoctial Points, and the Obliquity of the Ecliptic, as also the real Quantity of the annual Precession, to every 5th Degree of the Place of the Moon's Ascending Node, in the following Tables; just as they

they result from the Hypothesis, as at first laid down; it appearing, from what has already been remark'd, that these will be sufficiently exact for Practice in all Cases.

The Equation of the Equinoct. Points				
Ds \odot from γ	Sig. O Sig. VI	I VII	II VIII	Subit Add
0	"	"	"	0
5	00	13	19.6	30
10	20	13.0	20.5	25
15	3.9	14.5	21.2	20
20	5.8	16.0	21.8	15
25	7.7	17.3	22.2	10
30	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subit.	Sig. V	IV	III	Ds \odot from γ
Add	Sig. XI	X	IX	

The Equation of the Obliquity of the Ecliptick.				
Ds \odot from γ	Sig. O Sig. VI	I VII	II VIII	Add Subit
0	"	"	"	0
5	00	7.8	4.5	20
10	9.0	7.4	3.8	25
15	8.9	6.9	3.1	20
20	8.7	6.4	2.3	15
25	8.5	5.8	1.6	10
30	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add	Sig. V	IV	III	Ds \odot from γ
Subit.	Sig. XI	X	IX	

The Annual Precession of the Equinoctial Points.						
Ds \odot from γ	Sig. O	I	II	III	IV	V
0	"	"	"	"	"	"
5	58.0	57.0	54.2	50.3	46.5	43.7
10	57.9	56.6	53.6	49.7	46.0	43.4
15	57.9	56.2	53.0	49.0	45.5	43.2
20	57.7	55.7	52.3	48.4	45.0	43.0
25	57.5	55.2	51.7	47.7	44.5	42.8
30	57.3	54.7	51.0	47.1	44.1	42.8
30	57.0	54.2	50.3	46.5	43.7	42.7
	Sig. XI	X	IX	VIII	VII	VI
						Ds \odot from γ

Sir *Isaac Newton*, in determining the Quantity of the annual Precession from the Theory of Gravity, upon Supposition that the Equatorial is to the Polar Diameter of the Earth as 230 is to 229, finds the Sun's Action sufficient to produce a Precession of $9''\frac{1}{8}$ only; and, collecting from the Tides the Proportion between the Sun's Force and the Moon's to be as 1 to $4\frac{1}{2}$, he settles the mean Precession, resulting from their joint Actions, at $50''$. But since the Difference between the Polar and Equatorial Diameter is found, by the late Observations of the Gentlemen of the *Academy of Sciences*, to be greater than what Sir *Isaac* had computed it to be; the Precession, arising from the Sun's Action, must likewise be greater than what he has stated it at, nearly in the same Proportion. From whence it will follow, that the Moon's Force must bear a less Proportion to the Sun's than $4\frac{1}{2}$ to 1; and perhaps the *Phænomena*, which I have now been giving an Account of, will supply the best *Data* for settling this Matter.

As I apprehend, that the Observations already set down will be judged sufficient, to prove in general the Truth of the Hypothesis before advanced; I shall not trouble your Lordship with the Recital of more, that I made of Stars lying at greater Distances from the Zenith; those not being so proper, for the Reason before-mention'd, to establish the Point that I had chiefly in View. But as it may perhaps be of some Use to future Astronomers, to know what were the *mean* Differences of Declination, at a given Time, between some Stars, that lie nearly opposite to one another in Right Ascension, and not far from either of the *Colures*; I shall set down the Result of the Comparison of a few, that differ so little in Declination,
that

that I could determine the Quantity of that Difference with great Certainty.

By the *mean* of 64 Observations, that were made of α *Cassiopeæ* before the End of the Year 1728, I collect, after allowing for the Precession, Aberration and Nutation as in the foregoing Tables; that the *mean* Distance of this Star was $68''.7$ South of $34^{\circ}.55'$, on the 27th Day of *March* 1727. By a like Comparison of 40 Observations, taken of γ *Ursæ Majoris* during the same Interval of Time, I find this Star was, at the same time, $39''.6$ South of $34^{\circ}.45'$. I carefully measured, with the Screw of the Micrometer, the Distance between the Points, with which these Stars were compared; and found them to be $9'.59''$ from each other, or one Second less than they ought to have been. Hence it follows, that the *mean* Difference of Declination between these two Stars, was $10'.28''.1$, on the 27th Day of *March* 1727.

By the *mean* of 65 Observations, that were taken of β *Cassiopeæ*, before the End of the Year 1728, this Star was $25''.8$ North of $32^{\circ}.20'$, on the 27th Day of *March* 1727: and by the *mean* of 52 Observations, ϵ *Ursæ Majoris* was $87''.6$ South of $32^{\circ}.30'$ at the same time. The Distance between these Points was found to be $9'.59''.3$; from whence it follows, that the *mean* Difference of Declination between these two Stars was $11'.52''.7$ on *March* 27th 1727.

By the *mean* of 100 Observations, taken before the End of the Year 1728, the *mean* Distance of γ *Draconis* was $79''.8$ South of $38^{\circ}.25'$ on *March* 27th 1727; and by the *mean* of 35 Observations, the

the 35th *Camelopard. Hevel.* was South of the same Spot $76''.4$. So that the mean Polar Distance of γ *Draconis* was only $3''.4$ greater, than that of the 35th *Camelopard. Hevel.* but as the Equation for the Nutation, in both these Stars, was then near the *Maximum*, and to be applied with contrary Signs; the *Apparent* Polar Distance of γ *Draconis* was $21''.4$ greater, on the 27th Day of *March* 1727.

The Differences of the Polar Distances of the Stars, as here set down, may be presumed, both on account of the Radius of the Instrument and the Number of Observations, to be very exactly determined, to the Time when the Moon's Ascending Node was at the Beginning of *Aries*; and if a like Comparison be hereafter made, of Observations taken of the same Stars, near the same Position of the Moon's Nodes; future Astronomers may be enabled, to settle the Quantity of the mean Precession of the Equinox, so far as it affects the Declination of these Stars, with great Certainty: and they may likewise discover, by means of the Stars near the Solstitial Colure, from what Cause the apparent Change in the Obliquity of the Ecliptic really proceeds, if the mean Obliquity be found to diminish gradually.

The forementioned Points indeed can be settled only on the Supposition, that the angular Distances of these Stars do continue always the same, or that they have no real Motion in themselves; but are at Rest in Absolute Space. A Supposition, which though usually made by Astronomers, nevertheless seems to be founded on too uncertain Principles, to be admitted in all Cases. For if a Judgment may be formed, with Regard to this Matter, from the Result

sult of the Comparison of our best modern Observations, with such as were formerly made with any tolerable Degree of Exactness; there appears to have been a real Change in the Position of some of the fixed Stars, with respect to each other; and such, as seems independent of any Motion in our own System, and can only be referred to some Motion in the Stars themselves. *Arcturus* affords a strong Proof of this: for if its present Declination be compared with its Place, as determined either by *Tycho* or *Flamsteed*, the Difference will be found to be much greater, than what can be suspected to arise from the Uncertainty of their Observations.

It is reasonable to expect, that other Instances of the like kind must also occur among the great Number of the visible Stars; because their relative Positions may be alter'd by various means. For if our own Solar System be conceived to change its Place, with respect to Absolute Space; this might, in Process of Time, occasion an apparent Change in the angular Distances of the fixed Stars; and in such a Case, the Places of the nearest Stars being more affected, than of those that are very remote; their relative Positions might seem to alter; tho' the Stars themselves were really immoveable. And on the other Hand, if our own System be at Rest, and any of the Stars really in Motion, this might likewise vary their apparent Positions; and the more so, the nearer they are to us, or the swifter their Motions are, or the more proper the Direction of the Motion is, to be rendered perceptible by us. Since then the Relative Places of the Stars may be changed from such a Variety of Causes, considering that amazing Distance

tance at which it is certain some of them are placed, it may require the Observations of many Ages, to determine the Laws of the apparent Changes, even of a single Star: much more difficult therefore must it be; to settle the Laws relating to all the most remarkable Stars.

When the Causes, which affect the Places of all the Stars in general are known; such as the Precession, Aberration, and Nutation; it may be of singular Use, to examine nicely the relative Situations of particular Stars: and especially of those of the greatest Lustre, which, it may be presumed lie nearest to us, and may therefore be subject to more sensible Changes; either from their own Motions, or from that of our System. And if at the same time that the *brightest* Stars are compared with each other, we likewise determine the relative Positions of some of the *smallest* that appear near them, whose Places can be ascertained with sufficient Exactness; we may perhaps be able to judge to what Cause the Change, if any be observable, is owing. The Uncertainty that we are at present under, with respect to the Degree of Accuracy wherewith former Astronomers could observe, makes us unable to determine several Things, relating to the Subject that I am now speaking of: but the Improvements, which have of late Years been made in the Methods of finding the Places of the heavenly Bodies, are so great, that a few Years may hereafter be sufficient, to settle some Points, which cannot now be settled, by comparing even the earliest Observations with those of the latest.

It were to be wish'd therefore, that such Persons as are provided with proper Instruments, would attempt to determine, with great Care, the present relative Positions of several of the Principal Stars, in various Parts of the Heavens; especially of those, that are least affected by Refraction: *that* Cause having many times so uncertain an Influence on the Places of Objects, that are very remote from the Zenith; that wherever *It* is concerned, the Conclusions, deduced from Observations that are *much* affected by it, will always remain doubtful, and too precarious, in many Cases, to be relied upon.

The Advantages, arising from different Persons attempting to settle the same Points of Astronomy near the same time, are so much the greater; as a Concurrence in the Result, would remove all Suspicion of Incorrectness in the Instruments made use of. For which Reason, I esteem the curious *Apparatus* at *Shirburn Castle*, and the Observations there taken, as a most valuable *Criterion*, whereby I may judge of the Accuracy of those, that are made at the *Royal Observatory*; and as a Lover of Science I cannot but wish, that our Nation abounded with more frequent Examples, of Persons of like Rank and Ability with your Lordship, equally desirous of promoting *This*, as well as every other Branch of Natural Knowledge, that tends to the Honour and Benefit of our Country.

But were the Patrons of Arts and Sciences ever so numerous, the Subject of my present Letter is of such a nature, that I must beg Leave to address it to the *Lord of Shirburn Castle* and only as a most competent Judge of it, but as the *sole Per-*
son,

son, in this Nation, that hath Instruments proper to examine into the Truth of the Facts here related. And it is a particular Satisfaction to me, that after so long an Attendance upon these *Phænomena*, I am allowed the Honour of transmitting the Account of them to the Public, thro' your Lordship's Hands: as it gives me at the same time an Opportunity of professing the grateful Sense I shall ever retain, both of the signal Favours which I formerly received from the noble Earl your Father, and of the many recent Obligations conferr'd by yourself upon,

My LORD,

Your Lordship's

most obedient

humble Servant,

Greenwich, Dec. 31.

1747.

Ja. Bradley.

II. *A Letter to Martin Folkes Esq. LL. D. Pr. R. S. containing some Observations upon certain Shell-Fish (lodg'd in a large Stone brought from Mahon Harbour by Mr. Samuel More, Purser of the Sterling-Castle Man of War) from James Parsons M. D. F. R. S.*

Read Jan. 21. 1747. **T**HE Favour Mr. More and Mr. Gra-

ham have done the *Royal Society*, by their Present of the Stone containing the shell'd Fish, and called the *Dottle-Stone*, has given Occasion to the following Hints, to shew of what sort these Fish are, and that they are rare and curious to us.

Upon our first hearing of them by Mr. More's Letter to *Richard Graham* Esq; because it was said the Fish were lodged in a Stone, it was thought they were the same we know by the Name of *Pholades*, and of which there are Plenty upon our own Coasts; but I believe none of us had seen these before, nor had a Notion of any others being lodged in Stone but our *Pholades* above mention'd; whereas these seem to be peculiar to the Mediterranean Sea, since *Rondeletius*, and after him *Aldrovandus*, have given their Account and Figures of them, among others that are Inhabitants of the same Regions.

These two Authors call them simply *Pholades*, which Term is derived of the Greek Verb φαλέω, and signifies, to have a hiding Place; every Animal therefore

therefore that absconds in Earth or Stones might be called so too: Hence I think, that *φῶλας* is too vague and insignificant a Name for any particular Animal, and that some other, which has a nearer Relation to its generical Character ought to be given to it.

If *Aldrovandus* had seen those, which we as erroneously call *Pholades*, inclosed in their Cells, he would, no doubt, have call'd them by the same Name, and for the same Reason; but I am inclined to think he never saw the intire Fish, but only the Shell; because he gives a very imperfect Description of it, among several others, which he has Figures of also, and which he calls *Concha longa Authoris alie*, which follow an Account and Figure of the *Concha longa* of *Pliny*.

Dr. Lister and *Rumphius* also have Figures of this Mediterranean Fish, and, after that Author, call it *Pholas*: But since this Term barely denotes the Place of its Residence, let us endeavour to give it a proper Name, which may be done by considering its Similarity with some *Genus* already known.

The above-mention'd Gentleman says, the Stones are from half a hundred to four or five hundred weight each, lying at all Depths to twenty Feet under Water; full of Cells, each containing a single Fish, call'd by the Inhabitants the *Dottle Fish*, which Name he judiciously supposes to be a Corruption of the Word *Dactylus* from their Form. He also says, the Fish is of the same Nature with the common *Muscle*, but much more delicious, and that eating them is never attended, with those poisonous Symptoms, that have been often thought to be caus'd by eating *Muscles*.

The Shells are indeed, in all Respects but one, like the *Mytulus vulgaris*, or *common Muscle*; these being small at the Hinge-End, and having a broad thin Edge at the Opposite; whereas the former are nearly equal at both Ends, as well as strait and somewhat depressed; and as to the Structure of the Fish of both, they are alike, tho' with this small Difference, that the *Lingula* of the common Muscle is detached towards the Point, and that of the other is confin'd all along. I therefore submit it, whether either of the following Names would not properly express it,

Mytulus cylindroides, the cylindroid Muscle, or *Mytulus Dactyliiformis*, the *Date Muscle*. Its external Form conducing much to encourage the latter, which Mr. More has hinted at, in calling the Stone the *Dottle*, *Dotting*, or *Dating-Stone*; for as to the Place of its Residence, that belongs rather to its general natural History than its distinguishing Name.

Doctor *Lister*, and after him Monsieur *D'Argenville* have drawn our *Pholas* with five Shells; but we have some Reason to suspect they are only Bivalves; for, upon examining those inclosed in the *Specimen* before you, in Company with Mr. *Hill*, none of them appeared to have any more than two Shells. And in a Specimen (given to Mr. *Peter Collinson* by Sir *Charles Wager*) of one of these Fish, which lodg'd itself in the Bottom of a Ship, there were but two Shells found; which Shell together with the Piece of Wood, wherein it was lodged, I myself saw along with several other Friends in the above-mention'd Gentleman's Collection. Now these ought, in like manner, after its generical Character,

Character, to be ranged among the *Chamæ*; and as they have a *Proboscis* which none of the *Mytuli* have, I would also offer the following Name for this Fish;

Chama longa rugis asperis, alba. The long rough white *Chama*.

Mr. *Baker* has shewn me another Species of *Pholas*, which he lately took out of a Stone from the Coast of *Cornwal*, and which has more of the *Pectuncle* than any other kind, in its Form, *Cardo*, and shutting close; which the Shells just mention'd cannot do: Now these three distinct Kinds of Shell-Fish can never be said to be rightly called by the single Name of *Pholas*.

The common Objection to these Fish boring their Way into the Stones in which they are found, *viz.* that the Stones are first in a soft State, and so harden about them, may be obviated by the following Considerations: First, that in Mr. *More's* great Stone, when it was broken, there appear'd thro' its Substance several petrify'd fossil Shells; which clearly shew that its Formation was of an ancienter Date than the Age of these Muscles can admit of. Secondly, That the Holes on the Surface are narrower, in general, than the Cavity in which the Fish lies; and which demonstrates, that they enter young, and are capable of enlarging their Room as they grow bigger, by abrading the Sides of their Cells: And this is further apparent, by the sandy Matter found in the Bottoms of those Cells, which the Fish cannot well get rid of, when it happens, that the Orifice is higher than the Bottom; Abundance of which

Mr.

Mr. *Hill* and myself observed in some of the Holes; and which is easily thrown quite out, when the Ori-
fice is depending; for in these we observed none :
And this is further confirmed by what Dr. *Woodward*
relates in the first Volume of his Catalogue *, that
certain Pillars of white *Carrara* Marble taken out
of the Sea, on the Coast of *Leghorn*, after lying
there a Number of Years, were destroy'd by the
boring of these *Pholades*.

As to the Manner of their penetrating the Stones,
I cannot give the least Account of it; who am,

S I R,

Your most obedient Servant,

James Parsons.

III. *A Collection of the Electrical Experiments communicated to the Royal Society by Wm. Watson, F. R. S. read at several Meetings between October 29. 1747. and Jan. 21. following.*

I.

An Account of the Experiments made by several Gentlemen of the Royal Society, in order to discover whether or no the electrical Power, when the Conductors thereof were not supported by Electrics per se would be sensible at great Distances: With an Inquiry concerning the respective Velocities of Electricity and Sound: To which is added an Appendix, containing some further Inquiries into the Nature and Properties of Electricity.

Read Oct. 29. 1747. **I**N the Paper I did myself the Honour some time since to communicate to the Royal Society, I took notice, that, among the many other surprising Properties of Electricity, none was more remarkable, than that the electrical Power, accumulated in any non-electric Matter contained in a glass Phial, described upon its Explosion a Circuit through any Line of Substances non-electrical in a considerable

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ble Degree; if one End thereof was in Contact with the external Surface of this Phial, and the other End upon the Explosion touched either the electrified Gun-barrel, to which the Phial in charging was usually connected, or the iron Hook always fitted therein. This Circuit, where the non-electric Substances, which happen to be between the Outside of the Phial and its Hook, conduct Electricity equally well, is always described in the shortest manner possible; but if they conduct differently, this Circuit is always formed through the best Conductor, how great soever its Length is, rather than through one which conducts not so well, though of much less Extent.

It has been found, that in proportion as Bodies are susceptible of having Electricity excited in them by Friction, in that Proportion they are less fit to conduct it to other Bodies; in consequence whereof, of all the Substances we are acquainted with, Metals conduct best the electrical Powers; for which Reason the Circuit before spoken of is formed through them the most readily. Water likewise is an admirable Conductor; for the electrical Power makes no Difference between Solids and Fluids as such, but only as they are non-electric Matter.

In order to give an Idea of what is understood by this Circuit, we will mention an Example or two, from which all the other may naturally be deduced. If a Person stands upon a dry wooden Floor with a coated Phial ever so highly charged in one of his Hands, and if another Person, without touching the first, stands but six Inches from him, and touches the iron Hook of the Phial, neither of them

are shocked; because the Floor between them, tho' the Distance is so short, will not conduct the Electricity sufficiently quick. But if these two Persons tread upon a Piece of Wire laid between them, they each of them feel the electrical Commotion in that Arm, which touches the Phial and Hook, and in that Foot which treads upon the Wire; the Wire here conducting the Electricity quick enough, which the dry Floor would not. The Circuit is here formed by the coated Phial, its Hook, so much of the Bodies of these two Persons as formed a curve Line between the Wire, the Phial, and Hook, and the Wire between these Persons. If these Persons stand upon, or touch with any Part of their Bodies any Non-electrics, which readily conduct Electricity, the Circuit is completed, and the Effect is the same: And this is occasion'd by the short Space of Time, in which the loaded Phial is discharged, when any Matter of what kind soever readily conducting Electricity happens to be between the coated Phial and its Hook, and is so connected as to communicate with both upon the Discharge of the Phial.

Monsieur *le Mounier* the younger at *Paris*, in an Account transmitted to the *Royal Society*, takes notice of his feeling the Stroke of the electrified Phial along the Water of two of the Basons of the *Thuilleries* (the Surface of one of which is about an Acre) by means of an iron Chain which lay upon the Ground, and was stretched round half their Circumference.

Upon these Considerations it was conjectured, as no Circuit had as yet been found large enough so to dissipate the electrical Power as not to make it

perceptible, that if the non-electrical Conductors were properly disposed, an Observer might be made sensible of the electrical Commotion quite across the River *Thames*, by the Communication of no other Medium than the Water of that River. But as perhaps, in what relates to Electricity less than in any other Part of Natural Philosophy, we should draw Conclusions but from the Facts themselves, it was determined to make the Experiment.

The making this Experiment drew on many others, and as the Gentlemen concerned flatter themselves that they were made with some Degree of Attention and Accuracy, they thought it not improper to lay a Detail of all the Operations relating thereto, before the *Royal Society*.

In order to try whether or no the electrical Commotion would be perceptible across the *Thames*, it was absolutely necessary that a Line of non-electric Matter, equal in Length to the Breadth of the River, should be laid over it so as to touch the Water thereof in no Part of its Length; and the Bridge at *Westminster* was thought the most proper for that Purpose, where the Water from Shore to Shore was somewhat more than 400 Yards.

Accordingly on *Tuesday July 14, 1747*. to see the Success and assist in making the Experiment, there met *Martin Folkes Esq*; President of the *Royal Society*, the Right Honourable the Earl *Stanhope*, *Richard Graham Esq*; *Nicholas Mann Esq*; and myself, with proper Persons to execute what was required of them in the various Parts of these Experiments.

A Line of Wire was laid along the Bridge, not only through its whole Length, but likewise turning at the Abutments, reached down the stone-Steps on each Side of the River low enough for an Observer to dip into the Water an iron Rod held in his Hand. One of the Company then stood upon the Steps of the *Westminster* Shore holding this Wire in his left Hand, and an iron Rod touching the Water in his right: On the Steps facing the former upon the *Surry* Shore, another of the Company took hold of the Wire with his right Hand, and grasped with his left a large Phial almost filled with Filings of Iron, coated with Sheet-Lead, and highly electrified by a glass Globe properly disposed in a neighbouring House. A third Observer standing near the second dipped an iron Rod held in his left Hand into the Water, and touching the iron Hook of the charged Phial with a Finger of his right Hand, the Electricity snapped, and its Commotion was felt by all the three Observers, but much more by those upon the *Surry* Shore. The third Observer here was no otherwise necessary, than that the River being full, the Iron was not long enough to be fixed in the Mud upon the Shore, and therefore was in want of some Support. The Experiment was repeated several times, and the electrical Commotion felt across the River; but the Gentlemen present being much molested in their Operations by a great Concourse of People, who many times broke the conducting Wire, and otherwise greatly incommoded them, and the Evening growing too dark for the Observers on different Sides of the Water to see each other, they were prevented from

from diversifying the Experiments, as was intended, and only consider'd these Trials as a still further Encouragement for them to prosecute the Inquiry at a more favourable Opportunity.

Early therefore on *Saturday Morning July 18*, there met upon *Westminster-Bridge* the *President* of the *Royal Society*, the Right Honourable the Lord *Charles Cavendish*, *Richard Graham Esq*; Dr. *Bevis*, and myself, with proper Assistants. At the preceding Meeting, the electrical Machine's being placed at some Distance from the Water being found inconvenient, the following Alteration was made in the Disposition of the *Apparatus*.

A Room up two Pair of Stairs in a commodious House nearest the Bridge on the *Surry Shore* was provided, in which was placed the electrical Machine with the Gun-barrel suspended in silk Lines. From this Room, on account of its Height, the Signals on both Sides of the River were easily observable. The coated Phial before-mention'd with its iron Hook was placed upon the Seat of the Window of this Room, and communicated with the Gun-barrel by the means of a Piece of iron Wire. One Extremity of another Wire was likewise fixed into the Bottom of the leaden Coating of the Phial, whose other Extremity reached therefrom over the Bridge to the Steps upon the *Westminster Shore*, the Body of the Wire being placed as much as possible upon the Parapet of the Bridge. One or more Observers took each other by the Hand, the first of which must necessarily take the Wire in his left Hand, and the last, upon the proper Signal given, either dip his right Hand into the

the Water, or (which makes the Posture more agreeable) a Rod of Metal held therein. Another Wire having no Communication with any of the former, was let down from the before-mention'd Room, and down the Steps upon the *Surry* Shore: One Extremity of this Wire was held in the Hand of an Observer standing upon these Steps, who dipped an iron Rod held in his other Hand into the Water: To the other Extremity of this Wire was fastened a short iron Rod, with which, when the electrified Phial was sufficiently charged, and the Signal given, the Gun-barrel was to be touched.

The Gentlemen, by this Disposition of the *Apparatus*, proposed to examine principally these three Questions: First, whether or no the Observers standing on each Side of the River would perceive the electrical Commotion, each putting an iron Rod into the Water? Secondly, Whether or no the Observers on both Sides of the River would feel the electrical Commotion, when the Observer standing upon the *Westminster* Shore removed the iron Rod held in his Hand out of the Water? Thirdly, Whether or no the electrical Power was perceptible to the Observers on both Sides of the River, if the Observer upon the *Westminster* Shore dipped his Hand into a Pail of Water, which had no Communication with the Water of the *Thames*.

It was determined first, upon proper Signals, to discharge the electrified Phial in the manner before-mention'd, the Observers on each Side of the River holding the iron Rods in the Water, and this Experiment was to be repeated three times. This was attempted accordingly; and although the Observer

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on the *Surry* Shore was each time smartly struck, the *President* of the *Royal Society*, who observed with the utmost Attention upon the *Westminster* Shore, gave the Signal that he felt nothing. The Company was surpris'd at this Want of Success in the Experiment; but, upon examining the Wire, which was laid over the Bridge, it was found to have been broken by some Accident, after it had pass'd over about a fourth Part of the Bridge. The Wire being refitted, it was agreed to make the same Experiment six times more: This was done accordingly, and the electrical Commotion was felt each time by the Observers on both Sides of the Water, but much smarter by those on the *Surry* Side. It was then thought proper to repeat this Experiment three times more upon the Signal's being given: but, in making the first of these, the Observer in the Room with the Machine, discharged the electrified Phial, before the Observer upon the *Surry* Shore had dipped his iron Rod into the Water, and therefore no Effect was perceiv'd by the Observer on the opposite Shore. The electrified Phial therefore was again discharged three other times, and the Commotion felt by the Observers on both Sides of the River.

To examine the second Question, no other Alteration was necessary in the whole *Apparatus*, than that the Observer upon the *Westminster* Shore should not dip either his Hand, or the iron Rod held therein in the last Experiments, into the Water of the River. The electrified Phial then was discharged three times without its Effects being in the least perceiv'd by the Observers upon the *Westminster* Shore; those indeed on that of *Surry* felt the Shock as before.

In examining the third Question, the *Apparatus* was in all other Respects the same as in the last; except that the Observer upon the *Westminster* Shore had a Pail of Water placed upon a wooden Table, which stood upon the Stone Steps, and into which he was to put his right Hand upon the Signal's being given. This was accordingly done, and the electrified Phial being discharged three times, the electrical Commotion was felt as before by the Observer upon the *Surry* Shore; but not in the least by him on the *Westminster* Side, who held his Hand in the Pail of Water.

In all these Experiments, except in one before-mention'd, where the iron Rod was not in the Water, it was found, that whether the Observers on the *Westminster* Shore, upon the Discharge of the electrified Phial, did or did not feel its Effects, they were always perceiv'd not only in the Arms of those upon the *Surry* Shore, who formed a Line between the Extremity of the Wire there, and the Water of the River; but by any other Person, who standing upon the Stone Steps, even where they were not wet, touched the Wire with his Hand. They were likewise felt by a Person upon the *Westminster* Shore, standing upon the wet Stone Steps, who did not form Part of the Line between the Extremity of the conducting Wire and the Water, otherwise than by touching the Wire with his Fingers.

As was before-mention'd, the Observers upon the *Westminster* Shore did not feel the Effects of the discharged Phial near so strong as those on that of *Surry* in the first Set of these Experiments. When a Line was there form'd by the joining Hands of two or more Persons, the first of which, on account

of the Situation, held the conducting Wire in his left Hand, and the last touched the Water with an iron Rod held in his right, the Effects were most sensible in the left Arm of him who held the Wire : They were indeed manifestly felt by them all ; but this Feeling was not great enough to be called a Shock, but, as was very properly expressed by one of the Company, it resembled the Pulsation of a large Artery.

From the Examination of the first and second Questions it appeared, that the Observers upon the *Westminster* Shore were not sensible of the Effects of the Electricity, unless their Bodies described Part of the Circuit before spoken of ; and this Circuit here consisted of Part of the Gun-barrel of the electrifying Machine, the Wire going from this Gun-barrel to the iron Hook, the Phial itself, the tail Wire of this coated Phial which reached therefrom across the Bridge and down the Steps on the *Westminster* Shore, the Line of Observers between this Wire and the iron Rod which dipp'd in the Water there, this iron Rod, a supposed Line of Water drawn quite across the *Thames*, the Observers with their iron Rod on the *Surry* Shore, the iron Wire going from the right Hand of the last of these up into the Room where the electrifying Machine was placed, and the short iron Rod to which one Extremity of this Wire was joined, and with which, in making the Explosion, the Gun-barrel was touched. The Length of this Circuit, through which the Electricity was propagated was, at least 400 Yards, more than 400 Yards of which was formed by the Stream of the River.

From the Examination of the third Question it appeared, that the electrical Commotion would not be

be felt from the Observer dipping his Hand in Water only, unless that Water was so disposed as to become Part of the Circuit; and this Experiment was made, lest the contrary might be surmised.

The Observers upon the *Westminster* Shore not feeling the electrical Commotion equally strong with those of *Surry*, was judged to proceed from other Causes besides that of Distance. For it must be consider'd, that the conducting Wire was almost throughout its whole Length laid upon *Portland* Stone standing in Water. This Stone, being in a great Degree non-electric, is of itself a Conductor of Electricity: And this Stone standing in Water, no more of the Electricity was transmitted to the Observers on the *Westminster* Shore than that Proportion, wherein Iron is more non-electric, and, consequently, a better Conductor of Electricity than Stone. This was made more manifest, from observing that whether the conducting Wire upon the Bridge was broke or no, and, consequently, whether the Observers upon the *Westminster* Shore felt the electrical Commotion or no, not only the Observers upon the *Surry* Shore, who with their Wire form'd Part of the Line, felt the Shock in their Arms; but those Persons who only stood upon the Stone Steps there, and touched the Wire with their Fingers, felt the electrical Commotion in the Arm of that Hand which touched the Wire, and down their Legs. From whence, and from the Person before spoken of feeling the electrical Commotion standing upon the wet Stone Steps of the *Westminster* Shore, tho' not forming Part of the Line, but only touching the Wire with his Fingers, it was concluded, that, besides the large Circuit before spoken of, there were

formed several other subordinate Circuits between the same Steps of the *Surry* Shore, and the Bridge by means of the Water; whereby that Part of the electrical Power, felt by the Observers upon the *Surry* Side of the River, and not by those on the *Westminster* Side, was discharged.

Dr. *Bevis* having observed, and which was likewise tried here, that however well an electrified Phial was charged, its iron Hook would not fire the Vapours of warm Spirit of Wine held in a Spoon and applied thereto, if the Person who held the Phial, and he who held the Spoon did not take each other by the Hand, or have some other non-electrical Communication between them; it was therefore thought proper to try the Effects of Electricity upon some warm Spirit of Wine through the large Circuit before-mention'd. Accordingly the Observers being placed as before both upon the *Westminster* and *Surry* Shores, no other Alteration was made in the before-mention'd *Apparatus*, than that the Wire which connected the Gun-barrel with the iron Hook of the coated Phial being laid aside, the coated Phial itself was charged at the Gun-barrel, and then brought in the Hands of an Observer near the warm Spirits in the Spoon, which was placed upon the short iron Rod before-mention'd, which was connected with the Wire which went to the Observers upon the *Surry* Shore. Upon presenting properly the iron Hook of the charged Phial to the warm Spirit, it was instantly fired, and the electrical Commotion felt by the Observers on both Sides of the River.

It was then thought proper to try the Effects of the charged Phial upon the warm Spirit, when the
Wire

Wire was divided which was laid over the Bridge : Upon presenting the iron Hook to the Spirit, a sufficient Snap was given to the Spoon to fire the Spirit, but nothing so smart as in the former Experiment where the large Circuit was completed.

It was then tried, what the Effect would be upon the Spirit, if the charged Phial was divested of its long Wire which lay over the Bridge, and was only held in the Hand of an Observer ; whilst the Spoon with warm Spirit was placed in Contact of the iron Rod before mention'd, to which the Wire was connected, which went to the Observers upon the *Surry* Shore ; and the Spirit was fired with much the same Degree of Smartness as in the last Experiment.

In these and all the subsequent Operations, Wires were made use of to conduct the Electricity preferable to Chains, as it before by great Numbers of Experiments had been fully proved, that whatever Difference there was in the Bulk of the Conductor, that is to say, whether it were a small Wire, or a thick iron Bar, the electrical Strokes communicated thereby were equally strong : And it had been further observed, besides the Difficulty of procuring Chains of a requisite Length for the present Purposes, that the Stroke at the Gun-barrel, when the Electricity was conducted by a Chain, was *cæteris paribus* not so strong, as when that Power was conducted by a Wire. This was occasion'd by the Junctures of the Links of the Chain not being sufficiently close, which caused the Electricity in its Passage to snap and flash at the Junctures, where there was the least Separation ; and these lesser Snappings in the whole Length of the Chain lessen'd the great one of the Gun-barrel.

Encouraged.

Encouraged by the Success of these Trials, the Gentlemen were desirous of continuing their Inquiries, and of knowing whether or no the electrical Commotions were perceptible at a still greater Distance. The *New River* near *Stoke Newington* was thought most convenient for that Purpose; as at the Bottom of that Town, the Twinings of the River are so circumstanced, that from a Place which we will call *A* to another *B*, the Distance by Land is about 800 Feet, but the Course of the River is near 2000. From *A* to another Place, which we will call *C*, in a right Line is 2800 Feet, but the Course of the Water is near 8000 Feet.

Accordingly, on *Friday July 24. 1747* there met at *Stoke-Newington* the President of the *Royal Society*, the Right Honourable the Lord *Charles Cavendish*, the Rev. Mr. *Birch*, *James Burrow Esq;* *Peter Duval Esq;* Mr. *George Graham*, *Wm. Jones Esq;* *James Lever Esq;* Mr. *Newcome*, *Charles Stanhope Esq;* Mr. *Trembley* and myself, who were of the *Royal Society*, and Dr. *Bevis*. To this Gentleman the Company were much obliged, not only for his great Readiness in assisting in all the Operations, but likewise for the Use of his electrifying Machine, which from its Size was conveniently portable. This Machine was now placed in a Room up one Pair of Stairs in a House near *A*, and the Signals from thence might easily be perceived by the Observers both at *B* and *C*.

It was proposed, first to try the electrical Commotion by the same Observers as at *Westminster-Bridge*, from *A* to *B*, the Distance as before-men-

tion'd being about 800 Feet by Land, and 2000 by Water, in order if possible to determine the Difference of the Strength of the Electricity felt there, and at the Stone-Bridge at *Westminster*; the Difference of the Length of the 2 Circuits being about 400 Feet in Favour of that of the new River.

To make the Experiment, an iron Wire was fastened to the Coating of the glass Phial before-mention'd, and conducted from one of the Windows of the Room over the new River without touching the Water; and from thence to *B*, laying in its whole Length upon the Grass in the Meadows, except where it passed over a Hedge. At *B*, when the Explosion was to be made, one or more Observers were to take the Extremity of this Wire in one Hand, and touch the Water of the River as before with an iron Rod held in the other. Another Wire was let down from the other Window of the Room; one Extremity of which was joined to the short iron Rod mention'd in the former Experiments, the other was held in the Hand of an Observer at *A*, whose other Hand held an iron Rod dipp'd into the River.

It was absolutely necessary that these Wires should touch each other in no Part of their Length, otherwise the before-mention'd Circuit would upon the Explosion be completed from their first Contact,

When every thing was thus disposed, and the Signals given, the charged Phial was exploded eight times, and the electrical Commotion every time immanely felt by the Observers both at *A* and *B*. Whether the Line of Observers at *B* consisted of one or more, they were always struck, and that more sharply than at *Westminster-Bridge* under the same

same Circumstances. One of the Observers, taking the Wire in his Hand without having any Communication either with any of the other Gentlemen or the Water of the River, felt the Shock in his Feet.

It was then thought proper to make right Explosions without any other Alteration in the *Apparatus* than that the Observers at *B*, should stand in the Meadow at some Distance from the Water, without having any Communication therewith other than that furnished by the Ground. This was accordingly done, and the Stroke felt little if at all less than those last-mention'd. But the electrical Strokes being felt smartly at the Distance of at least 20 Feet from the Water occasion'd a very perplexing Difficulty, as it was impossible by this Experiment to determine with any Certainty, whether or no the electrical Circuit was formed throughout the Windings of the River, or much shorter by the Ground of the Meadows. The Experiment plainly shewed that the Meadow-Ground with the Grass thereon conducted the Electricity better than Stone; as it must be remember'd, that the Observers upon the Stone Steps upon the *Westminster* Shore felt not in the least degree the electrical Commotion, when their iron Rod was not in the Water, and themselves stood upon the dry Stone Steps. But this Effect was supposed to be owing to the Meadow Ground here being encompassed on two Sides by the *New River*, and on the other by a wet Ditch, by both which it was generally well moisten'd. To solve therefore this Difficulty a Series of Experiments were executed, of which hereafter.

The Gentlemen then determin'd to examine whether the electrical Commotions were perceptible from

Commutations were perceptible from *A* to *C*; a Distance not less than 2800 Feet by Land, and near 8000 by Water.

To execute this, to the former Wire, which was already conducted to *B*, another was added, which there crossed the River without touching the Water; and reached almost to *C*, where the first of a Line of Gentlemen held as before the Wire in one Hand, and the last dipp'd the Iron into the Water. The Wire from the Machine to *A* was as before. Upon the Signal's being given, the charged Phial was exploded ten times, and its Effects plainly though but faintly perceived each time by some or other of the Observers, but never by them all. The electrical Commotion was always felt by that Observer, who held the Extremity of the Wire, but never by him who held the iron Rod in the Water. It was in one Experiment felt by the Observer who held the Wire, not felt by the next who held the Hand of the former, and yet plainly perceived by the third who joined the second. Those who did not themselves feel the electrical Commotion here, did as at *B* see the involuntary Motions of those who did. The Observers at *A* felt the Shocks in the same Degree, whether the other Observers were station'd at *B* or *C*.

This Experiment further demonstrates the Distance to which the electrical Power may be conveyed: but the same Difficulty occurs here as in the last; to wit, whether the Circuit was completed by the Ground, or by the Water of the River?

These same Operations, which shewed at how great a Distance the electrical Commotion was perceptible,

ceptible, solved likewise three Questions of a subordinate Nature.

First, whether or no, *ceteris paribus*, any Difference occurred in the Success of the Experiment, if the long Wire, instead of being joined to the Coating of the Phial, was fasten'd to the short iron Rod, which upon touching the Gun-barrel occasion'd the Explosion; and if the short Wire, which only went to the Observer at *A*, a Distance from the Machine not more than 30 Feet, was joined to the Coating of the Phial? Upon Trial no Difference * was found.

Secondly, Whether or no, *ceteris paribus*, any Difference in the electrical Commotion would be perceived, when that Power passes through the Arms of two Observers, whose Bodies made Part of the Circuit, standing in the Room near the electrifying Machine; one of which takes the Extremity of the Wire that goes to the Observer at *A* in one Hand, and touches the Gun-barrel with the short iron Rod held in his other Hand? The other Observer takes the Extremity of the Wire which goes to *B* or *C* in one Hand, and touches the Coating of the charged Phial with his other. In several Trials, where each of these Observers frequently changed Stations, no Difference in point of Strength was observed in the electrical Commotion.

Thirdly,

* No Difference is observed when the electrical Circuit is propagated through Substances which readily conduct Electricity; if they conduct it in a less Degree, the electrical Commotion is most perceptible to the Observer, who holds the Wire, which comes from the charged Phial.

Thirdly, Whether or no these two Observers last-mention'd received the Shock at the same time? They were seen to be both convulsed in the same Instant.

July 28. 1747, there met again at the same Place, to proceed further in these Inquiries, the *President* of the *Royal Society*, the Right Honourable the Lord *Charles Cavendish*, the Reverend Mr. *Birch*, Sir *Francis Dashwood* Baronet, *Peter Daual* Esq; Mr. *Ellicott*, Mr. *George Graham*, *Richard Graham* Esq; Mr. *Robins*, Mr. *Short*, Dr. *Wilbraham*, and myself, who were of the *Royal Society*, and Dr. *Bervis*.

The electrical Commotion was first tried from *A* to *B* before-mention'd, the iron Wire in its whole Length being supported, without any-where touching the Ground, by dry Sticks placed at proper Intervals of about three Feet in Height. The Observers both at *A* and *B* stood upon Originally-Electrics, and, upon the Signal, dipped their iron Rods into the Water. Upon discharging the Phial, which was several times done, they were both very much shocked, much more so than when the conducting Wires lay upon the Ground, and the Observers stood thereon, as in the former Experiments. The same Experiment was tried with the Observer at *A*, instead of the iron Rod, dipping a narrow Slab of *Portland Stone* into the Water of about three Feet and a half in Length; when the Shock was felt, but not so severe as through the iron Rod. This demonstrated, as was before suggested, why the electrical Commotion was not felt stronger by the Observers upon the Western Shore of the *Westminster-Bridge*;

Bridge; viz. that *Portland* Stone standing in Water will conduct Electricity very considerably.

The Gentlemen then tried what would be the Effect, if the Observer at *B* stood upon a Cake of Wax holding the Wire as before, and touched the Ground of the Meadow with his iron Rod at least 150 Feet from the Water; and if the Observer usually placed near the River at *A*, had his Wire carried 150 Feet over the River as the former, stood upon an Originally-Electric, and touched the Ground with his iron Rod. Upon the Explosion of the charged Phial, which was several times done, both the Observers were smartly struck: This demonstrated, that in these Instances the moist Ground of the Meadows made Part of the Circuit. The Observers were distant from each other about 500 Feet.

The Observers then, station'd as in the last Experiment, stood upon the wax Cakes as before, without touching the Ground with the iron Rods, or any Part of their Bodies, and the charged Phial was exploded four times. These were not at all felt by the Observer next to *B*, and without the greatest Attention would not have been perceived by him next to *A*; and then only in some of the Trials, the Feeling of the Electricity was like that of a small Pulse between the Finger and Thumb of that Hand which held the Wire. The loaded Phial was again discharged four times more, without any other Alteration in the Disposition of the *Apparatus* than that the Observer next to *B* stood upon the Ground; when the electrical Commotion was perceived by that Observer, though not so sharp as when the other Observer at the same time stood
upon

upon the Ground. The Observer next to *A* felt the Tingling between his Finger and Thumb as before.

The Gentlemen were desirous of trying the electrical Commotion at a still greater Distance than any of the former through the Water, and where, at the same time by altering the Disposition of the *Apparatus*, it might be tried, whether or no that Power would be perceptible through the dry Ground *only* at a considerable Distance. *Highbury* Barn beyond *Islington* was thought a convenient Place for this Purpose, as it was situated upon a Hill nearly in a Line, and almost equidistant from two Stations upon the *New River*, somewhat more than a Mile asunder by Land, though following the Course of that River their Distance from each other was two Miles. The Hill between these Stations was of a gravelly Soil; which, from the late Continuance of hot Weather without Rain, was dry, full of Cracks, and consequently was as proper to determine whether or no the Electricity would be conducted by dry Ground to any great Distance, as could be desired. This hitherto had not been attempted; the Meadows in the Instances before quoted conducting the Electricity was supposed to be owing to the Moisture of the Ground. The Streets of *London*, when very dry, had been found to conduct it strongly about forty Yards, and the dry Road at *Newington* about the same Distance. Accordingly, on *Weanesday, Aug. 5. 1747.* there met at *Highbury-Barn* the Right Honourable the Lord *Charles Cavendish*, the Reverend Mr. *Birch*, Mr. *George Graham*, *Richard Graham* Esq; N. *Mann* Esq; Mr. *Skart*,

Short, Daniel-Wray Esq; and myself, who were of the *Royal Society*, and *Dr. Bevis*.

The electrifying Machine being placed up one Pair of the Stairs in the House at *Highbury-Barn*, a Wire from the coated Phial was conducted upon dry Sticks as before to that Station by the Side of the *New River*, which was to the Northward of the House. The Length of this Wire was 3 Furlongs and 6 Chains, or 2376 Feet. Another Wire fasten'd to the iron Bar, with which, in making the Explosion, the Gun-barrel was touched, was conducted in like manner to the Station upon the *New River* to the Southward of the House. The Length of this Wire was 4 Furlongs 5 Chains and 2 Poles, or 3003 Feet. The Length of both Wires, exclusive of their Turnings round the Sticks, was 1 Mile 1 Chain and 2 Poles, or 5379 Feet. For the more conveniently describing the Experiments made here, we will call the Station to the Northward *D*, and the other *E*.

At this Distance the Gentlemen propos'd to try, first, Whether or no the electrical Commotion was perceptible, if both the Observers at *D* and *E*, supported by Originally-Electrics, touched the conducting Wire with one Hand, and the Water of the *New River* with an iron Rod held in the other? Secondly, Whether or no that Commotion was perceptible, if the Observer at *E*, being in all respects as before, the Observer at *D*, standing upon Wax, took his Rod out of the Water? Thirdly, Whether or no that Commotion was perceptible to both Observers, if the Observer at *D* was placed upon Wax,
and

and touched the Ground with his iron Rod in a dry gravelly Field at least 300 Yards from the Water?

As from the Situation of the Ground, Trees, &c. neither of the Stations could be seen by each other, or by the Observer at the electrifying Machine, it was agreed to discharge a Gun as a Signal to get ready, and to do the same, as near as might be, half a Minute before each Explosion.

In these Experiments, as well as the former, the coated Phial was each time charged as high as it could be; so that if the Difference of the Shock to the Observers was considerable, it was owing to other Causes more than to the Phial's being differently electrified.

To try the first Proposition, eight Explosions were made with the Observers at *D* and *E*, touching the Water, and standing upon Wax, with their iron Rods in the Water. The first two of these were felt but weakly by the Observer at *D*; but in the other six he was strongly shocked. The Observer at *E* felt nothing of the first six Explosions; when, upon Examination, the Wire was found broken by some Accident; but this Observer was strongly shocked by the two last. The Observer at *D* being shocked in four of these Explosions, while in these four the Observer at *E* felt nothing, was owing to the Circuits being formed by the Ground between the Observer at *D* and the broken Wire. Upon account of the Wire's being broken, the Gentlemen tried three more Explosions, when the Observers at both Stations felt the electrical Shock.

To try the second Proposition, four Explosions were made with the Observer at *D* standing upon

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an Originally-Electric, and taking his iron Rod out of the Water, the Observer at *E* as before. In each of these the Observer at *D* felt a small Pulsation between his Finger and Thumb of that Hand, which held the Wire. The Observer at *E* felt each of these as strong as before. This being different from the Observations made in the Experiments of the last Trials at our former Stations *A* and *B*, and many others; where *B* in the same Circumstances with *E* here felt the electrical Commotion only in a slight Degree, was owing, as we were afterwards informed, to the impertinent Curiosity of the Servants of the Gentlemen, and other voluntary Observers, who, by touching the Wire which went from the coated Phial to the Observer at *D*, felt the Shock in their Arms and Ankles, and formed subordinate Circuits to *E*. The preventing these People from touching the Wires, was impossible; as great Part of them could be seen neither by the Observers at the Stations, nor by those at the House, and their being more than a Mile long.

The four other Explosions were made without any other Alteration in the *Apparatus*, than that the Observer at *D* stood upon the Ground about four Yards from the Water without any Communication therewith. The Observer at *E* felt the Shocks in his Arms as before; but the Observer at *D* standing upon the Ground was shocked in the Elbow and Wrist of that Arm which held the Wire, and in both his Ankles.

To try the third Proposition, eight Explosions were made with the Observer at *D* standing upon an Originally-Electric with his Rod in the Water of
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the River as before; but the Observer at *E* was placed in a dry gravelly Field about 300 Yards nearer the Machine than his last Station, and about 100 Yards distant from the River. He there stood upon the Wax, holding the conducting Wire in one Hand, and touched the Ground with an iron Rod held in the other. The Shock was each time felt by the Observer at *D*, but sensibly weaker than in the former Trials; but the Observer at *E* felt them all equally strong with the former; the four first in his Arms, when he stood upon the Wax, and touched the Ground with his iron Rod; the other four in his Arm and Ankles, when he stood upon the Ground without the iron Rod.

In some of these Experiments, the Observers at *D* felt a Tingling as soon as they laid hold of the conducting Wire. This was conjectured to be owing to the Electricity, which constantly runs off while the coated Phial is filling, and preferably by the Wire, as the best Conductor.

From the Severity of the Shock, the Gentlemen, in some of these Trials, did not choose to have the Electricity pass through their Bodies: But, as it was necessary for them to be sensible of the different Degrees of the electrical Commotions, they bound the conducting Wire round one of their Thumbs, and touched the iron Rod with the Fore-finger of the same Hand; when the electrical Commotion was felt only in so much of the Finger and Thumb of that Hand, as completed the Circuit.

In the Experiments of this Day, the Gentlemen were satisfied, that the dry gravelly Ground conducted the Electricity as strongly as Water, which, though otherwise

otherwise at first conjectured, they now found not to be necessary to convey that Power to great Distances ; as well as that, from Difference of Distance only, the Force of the electrical Commotion was very little if at all impaired. They were convinced of the Truth of the first of these Facts, not only from both Observers feeling the electrical Commotion in the eight last Experiments, when the Observer at *E* was at such a Distance from the Water, but also from the Observer at *D* feeling the Shock so strong in four of the first six Explosions, when the conducting Wire to *E* being broke at about 100 Yards Distance from the House, that Observer felt nothing.

In this last Instance the Circuit was formed from the Phial by the Observer at *D* and his Wire, a Line of Ground which reached from the Station at *D* to the broken Wire that lay upon the Ground, and so much of this Wire as reached to the short iron Rod, which touched the Gun-barrel in making the Explosions. This induced the Gentlemen to conclude (as from many Experiments it was manifest, that when the intervening Substances conduct Electricity equally well, the Circuit was performed in the shortest manner possible), that when the Observers holding their iron Rods in the River at *D* and *E* were both shocked, the Electricity was not conveyed by the Water of the River, being two Miles in Length, but by Land, where the Distance was only one Mile ; in which Space that Power must necessarily pass over the *New River* twice, through several Gravel Pits, and a large Stubble-Field. So that, admitting the Electricity did not
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follow the Tract of the River, the Circuit from *D* to *E* was at least two Miles; *viz.* somewhat more than one Mile of Wire, which conducted the Electricity from the House to the Stations, and another Mile of Ground, the shortest Distance between those Stations. The same Inference was now drawn with regard to the Experiments at *A, B,* and *C,* in the *New River* before recited; *viz.* that as in all of them the Distance between the Observers was much greater by Water than by Land, the Electricity passed by Land from one Observer to the other, and not by Water.

From the Shocks which the Gentlemen received in their Bodies, when the electrical Power was conducted upon dry Sticks, they were of Opinion, that from Difference of Distance simply consider'd, as far as they had yet experienced, the Force thereof was very little if at all impaired. When they stood upon Originally-Electrics, and touched the Water or Ground with an iron Rod, the electrical Commotion was always felt in their Arms and Wrists: When they stood upon the Ground, and touched either the Water or Ground with their iron Rods, they felt the Shock in their Elbows, Wrists, and Ankles: When they stood upon the Ground without the Rod, the Shock was always in the Elbow and Wrist of that Hand, which held the conducting Wire, and in both Ankles. The Observers here being sensible of the electrical Commotion in different Parts of their Bodies, was owing in the first Instance to the Whole, of its passing (because the Observer stood upon ~~Water~~) through their Arms, and through the iron Rod: In the second, when they stood upon the Ground, the

the Electricity passed both through their Legs, and thro' the Iron : In the third, when they stood upon the Ground without either Wax or Rod, the Electricity directed its Way through one Arm, and through both Legs to complete the Circuit.

The Gentlemen were desirous of closing the present Inquiry, by examining not only whether or no the electrical Commotions were perceptible at double the Distance of the last Experiments in Ground perfectly dry, and where no Water was near ; but also, if possible, to distinguish the respective Velocities of Electricity and Sound. To execute this, required the whole Sagacity and Address of the Gentlemen concerned ; for they had met with very great Difficulties in the last Day's Operations, where the Wire was conducted but little more than a Mile ; all which could not but be greatly augmented by doubling that Distance ; because it was necessary, that the House, wherein the electrifying Machine was placed, should be visible at least at one of the Stations ; and that the Space between that House and the Stations, through which the Wire was conducted, should be very little intersected by Hedges, Roads, or Foot-paths ; neither should the Wire in this Space be subject to be disturbed by the Horses or Cattle, which were grazing ; nor ought it to touch in its Passage the Trees or any other Vegetables, which at this Season of the Year were every where luxuriant. To find a Place within a convenient Distance of *London* with these Requisites was not very easy ; but at last, *Shooters Hill* was pitched upon, as the most convenient.

As only one Shower of Rain had fallen during the preceding five Weeks, the Ground could not but be very dry ; and as no Water was near, if the electrical Commotion was felt by the Observers at the Stations, it might be safely concluded, that Water had no Share in conducting it.

August 14. 1747. there met at *Shooters Hill* for this Purpose, the Rev. Mr. *Birch*, the Rev. Mr. Professor *Bradley*, *Peter Daval* Esq; Mr. *George Graham*, *R. Graham* Esq; Mr. *Nourse*, *George Lewis Scott* Esq; Mr. *Short*, *Charles Stanhope* Esq; and myself; who were of the *Royal Society*, and Dr. *Bevis*.

It was here determin'd (as the Gentlemen were satisfied from many of the former Trials, that if, when the coated Phial was discharged, the Observers at the Stations stood upon Originally-Electrics, and touched neither Water nor Ground with iron Rods, or any Part of their Bodies, the electrical Commotion would be scarcely perceptible) to make twelve Explosions of the coated Phial, with an Observer placed at the seven Mile-Stone, and another at the nine Mile-Stone, both standing upon Wax, and touching the Ground with an iron Rod. This Number of Explosions was thought more necessary, as the Observers at these Stations were not only to examine whether or no the Electricity would be propagated to so great a Distance; but if were, the Observer at the seven Mile-Stone was by a second Watch to take notice of the Time lapsed between feeling the electrical Commotion, and hearing the Report of a Gun fired near the Machine, as close as might be to the Instant of making the Explosion :

Explosion: And therefore, to examine this Matter with the requisite Exactness, this Number of Explosions should be made.

To execute this, the electrifying Machine was placed up one Pair of Stairs in a House upon the West Side of *Shooters Hill*; and a Wire from the short iron Rod, with which the Gun-barrel was touched in making the Explosions was conducted upon dry Sticks as before into a Field near the seven Mile-Stone. The Length of this Wire, exclusive of its Turnings round the Sticks, was a Mile, a Quarter and eight Poles, or 6732 Feet. In great Part of this Space it was found very difficult to support the Wire, on account of our scarcely being able to fix the Sticks in the strong Gravel there almost without any Cover of Soil; nor could the Wire in some Places be prevented from touching the Brambles and Bushes, nor in one Field the ripe Barley.

Another Wire was likewise conducted upon Sticks from the coated Phial to the nine Mile-Stone. In this Space, the Soil being a strong Clay, the Wire was very well secured, and in its whole Length did not touch the Bushes. The Length of this Wire was 2808 Feet. As much as the Place, where the Observers were station'd in a Corn-Field, was nearer the Machine than the seven Mile-Stone, so much were the other Observers placed beyond the nine Mile-Stone, that their Distance from each other might be two Miles. The forty Feet of Wire in these two Measures exceeding two Miles, was what connected the short iron Rod before mention'd, and the coated Phial with their respective conducting Wires.

The Observers being placed at their respective Stations, the Observer at the Machine proceeded in making the Explosions of the coated Phial; he having before placed an Assistant exactly in his View before the Window of the House, who, upon the Word of Command, was to discharge a Musket. As soon as ever the Flash was seen to come from the Mouth of the Gun, the Observer discharged the electrified Phial. When eight Explosions had been made, a Servant was sent from the Gentlemen at the seven Mile-Stone giving an Account of the Wire's being broken, and the Sticks thrown down by a Man riding through them; that the Observers there had felt nothing; and desired, as by this time the Wire was replaced, that we should begin again. This was complied with; and twelve other Explosions made without further Molestation.

Not only the first eight, but eleven of the last twelve very strongly shocked the Observers at the nine Mile-Stone: At the twelfth Explosion the Observer on purpose stood upon the Wax without touching the Ground with his iron Rod, or any Part of his Body; and only felt a slight Tingling in his Finger and Thumb that held the Wire. In another of these Experiments, as the Gentlemen here were satisfied in their own Persons of the Strength of the electrical Commotion, they indulged two Country Fellows, who were By-standers, with feeling one: These two with four of the Gentlemen formed a Chain, the first of them taking hold of the Extremity of the Wire with one of his Hands. They all stood upon the Ground, and made no Use of

of the iron Rod. Upon the Explosion they were all so strongly shocked in their Arms and Ankles, that the Countrymen could by no means be prevailed upon to try the Experiment again. Why, in the first eight Explosions, the Observers here were sensible of the electrical Commotion, when the Observers at the other Station felt nothing, was explained in the former Experiments. The Observers at this Station, from their Situation under the Hill, and from what Wind there was being against it, never heard the Report of the Gun.

Though the Observers near the seven Mile-Stone from the breaking of their Wire, were not sensible of the eight first Explosions of the charged Phial, they felt the other twelve. This demonstrated to the Satisfaction of the Gentlemen concerned, that the Circuit here formed by the Electricity was four Miles; *viz.* two Miles of Wire, and two Miles of Ground, the Space between the Extremities of that Wire. A Distance without Trial too great to be credited! How much further the electrical Commotion will be perceptible, future Observations can only determine.

The electrical Commotion by the Observers near the seven Mile-Stone was but slightly felt; nor could it be otherwise expected, the Wire in many Parts of its Length touching, as was before mentioned, the moist Vegetables; which, in as many Places as they were touched, formed subordinate Circuits. We find, in all other Instances, that the whole Quantity of Electricity, accumulated in the coated Phial, is felt equally through the whole Circuit, when

even

every Part thereof is in a great degree non-electric; so here the whole Quantity, or nearly so †, determined that Way, was felt by the Observers at the nine Mile-Stone; whilst those at the other Station felt so much of their Quantity only, as did not go through the Vegetables; that is, that Proportion only in which Iron is a greater Non-electric than the Vegetables.

Tho' the electrical Commotions, felt by the Observers near the seven Mile-Stone, were not strong; they were equally conclusive in shewing the Difference between the respective Velocities of Electricity and Sound.

The Space through which Sound is propagated in a given Time, has been very differently estimated by the Authors, who have wrote concerning this Subject. *Roberval* gives it at the Rate of 560 Feet in a Second; *Gassendus*, at 1473; *Mersenne* at 1474; *Du Hamel*, in the History of the *Academy of Sciences* at *Paris*, at 1172; the *Academy del Cimento*, at 1185; *Boyle* at 1200; *Roberts* at 1300; *Walker* at 1338; *Sir Isaac Newton* at 968; *Dr. Derham*, in whose Measure *Mr. Flamsteed* and *Dr. Halley* acquiesced, at 1142. But by the Accounts since published by *M. Cassini de Thury* in the *Memoirs of the Royal Academy of Sciences* at *Paris* for the Year 1738. where Cannon were fired at various as well as great Distances, under great Variety of Weather, Wind, and other Circumstances, and

† The Author of this Paper, from a great Variety of Experiments, is of Opinion; that in this and the like Dispositions of the Apparatus, the electrical Power, accumulated in the Matter contained in the coated Phial, is directed upon the Explosion thereof towards both Observers at the same Instant.

and where the Measures of the different Places had been settled with the utmost Exactness, Sound was propagated at a *Medium* at the Rate only of 1038 *French* Feet in a Second. The *French* Foot exceeds the *English* by seven Lines and a half, or is as 107 to 114: And consequently 1038 *French* Feet are equal to 1106 *English* Feet. The Difference therefore of the Measures of Dr. *Derham* and M. *Cassini* is $\frac{34 \text{ French}}{36 \text{ English}}$ Feet in a Second.† According to this last Measure, the Velocity of Sound, when the * Wind is still, is settled at the Rate of a Mile, or 5280 *English* Feet in $4'' \frac{77}{100}$.

To return to our Purpose; the Length of the conducting Wire from the Machine to the Observers near the seven Mile-Stone was (as has been before-mention'd) a Mile, a Quarter, and 8 Poles, or 6732 Feet: The Length of that to the nine Mile-Stone, 3868 Feet. The first of these Measures only was made use of in the present Operations concerning the Velocity of Electricity. In twelve Discharges of the coated Phial, which were felt by Mr. *George Graham*, Mr. *Short*, and *Charles Stanhope* Esq; the Observers near the seven Mile-Stone, and who, by a second Watch of Mr. *Graham's*, measured the
Time

† M. *Cassini de Thury* afterwards measured the Velocity of Sound at *Aiguemortes* in *Languedoc*, and found the Observations there from those made about *Paris* vary only half a Toise in a Second. See *Mem. de l'Acad. Royale des Sciences, pour l'année 1739*, p. 126.

* Dr. *Derham* found, that when Sound was carried against the Wind, not only its Distance but its Velocity was lessen'd; and in M. *Cassini's* Memoir, there is an Experiment, where Sound being carried against the Wind, which then blew very strong, was retarded near a twelfth Part of the usual Time in its Progress.

Time between feeling the electrical Commotion, and hearing the Report of the Gun, with the utmost Attention and Exactness; the Time, I say, between feeling the electrical Commotion, and hearing the Report of the Gun, was, at a *Medium*, 5 Seconds and a Quarter, or $5'' \frac{250}{1000}$. And as the Gun was distant from these Observers 6732 Feet, it follows, from the Experiments, which have been made on the Velocity of Sound, that the real Instant of the Discharge of the Gun preceded that of the Observers hearing its Report, at this time when the Strength of the Wind was not so great as to enter into the Computation, $6'' \frac{87}{1000}$; or preceded the Instant when the electrical Commotion was felt only $0'' \frac{837}{1000}$. But this Instant was, from the Nature of the Experiment, necessarily prior to that of the electrical Explosion, which was not made till the Fire of the Gun was actually seen; and therefore the Time between the making of that Explosion, and its being actually felt by the Observer, which must have been less than $0'' \frac{837}{1000}$, was really so small, as not to fall under any certain Observation, when it is to be distinguished from that, which must of Necessity be lost, between the Firing of the Gun, and the electrical Explosion itself.

In all the Experiments, where the Circuit was formed to any considerable Length, though the coated Phial was very well charged, the Snap at the Gun-barrel, upon the Explosion, was not near so loud as when the Circuit is formed in a Room; so that a By-stander, though versed in these Operations, from seeing the Flash, and hearing the Report, would imagine the Stroke at the Ends of the conducting Wire to be very slight; the contrary whereof,

when the Wire has been properly conducted, has always happen'd.

From a Review of these Experiments, the following Observations may be deduced.

- I. That, in all the preceding Operations, when the Wires have been properly conducted, the electrical Commotions from the charged Phial have been very considerable only, when the Observers at the Extremities of the Wire have touched some Substance readily conducting Electricity with some Part of their Bodies,
- II. That the electrical Commotion is always felt most sensibly in those Parts of the Bodies of the Observers, which are between the conducting Wires, and the nearest and the most non-electric Substance; or in other Words, so much of their Bodies, as comes within the electrical Circuit.
- III. That, upon these Considerations, we infer, that the electrical Power is conducted between these Observers by any non electric Substances, which happen to be situated between them, and contribute to form the electrical Circuit.
- IV. That the electrical Commotion has been perceptible to two or more Observers at considerable Distances from each other, even as far as two Miles.
- V. That when the Observers have been shocked at the End of two Miles of Wire, we infer, that the electrical Circuit is four Miles; *viz.* two Miles of Wire, and the Space of two Miles of the non-electric Matter

Matter between the Observers, whether it be Water, Earth, or both.

VI. That the electrical Commotion is equally strong, whether it is conducted by Water or dry Ground.

VII. That if the Wires, between the electrifying Machine and the Observers, are conducted upon dry Sticks, or other Substances non-electric in a slight Degree only, the Effects of the electrical Power are much greater than when the Wires in their Progress touch the Ground, moist Vegetables, or other Substances in a great Degree non-electric.

VIII. That by comparing the respective Velocities of Electricity and Sound; that of Electricity, in any of the Distances yet experienced, is nearly instantaneous.

I shall conclude this Paper with observing, that it was thought convenient to lay a Detail of all the Operations relating to these Experiments before the *Society*; in consequence of which the Gentlemen may make themselves Judges, how far the Deductions here recited are warrantable from the Experiments.

* The Gentlemen concerned were desirous, if possible, of ascertaining the absolute Velocity of Electricity

* These Experiments to measure the absolute Velocity of Electricity were made whilst this Paper was at the Press, but as they had so near a Relation to the Experiments made the preceding Year, it was thought proper to insert them here.

Electricity at a certain Distance; because, although last Year, in measuring the respective Velocities of Electricity and Sound, the Time of its Progress was found to be very little, yet we were desirous of knowing, small as that Time was, whether it was measurable; and I had thought of a Method for this Purpose.

Accordingly, *August 5. 1748.* there met at *Shooter's Hill* for this Purpose the *President* of the *Royal Society*, the *Rev. Mr. Birch*, the *Rev. Mr. Professor Bradley*, *James Burrow Esq*; *Mr. Ellicot*, *Mr. George Graham*, *Richard Graham Esq*; the *Rev. Mr. Lawrie*, *Charles Stanhope Esq*; and myself, who were of the *Royal Society*, *Dr. Bevis*, and *Mr. Grifchow* a Member of the *Royal Academy of Sciences at Berlin*.

It was agreed to make the electrical Circuit of two Miles, in the middle of which an Observer was to take in each Hand one of the Extremities of a Wire, which was a Mile in Length. These Wires were to be so disposed, that this Observer being placed upon the Floor of the Room near the electrifying Machine, the other Observers might be able in the same View to see the Explosion of the charged Phial and the Observer holding the Wires, and might take notice of the Time lapsed between the discharging the Phial and the convulsive Motions of the Arms of the Observer in consequence thereof; inasmuch as this Time would shew the Velocity of Electricity, through a Space equal to the Length of the Wire between the coated Phial and this Observer.

The electrifying Machine was placed in the same House as it was last Year. We then found ourselves greatly embarrassed by the Wire's being conducted by the Side of the Road, which we were compell'd to, on account of the Space necessary for the measuring of Sound: But so great a Distance from the Machine was not now wanted, though the Circuit through the Wire was intended to be at least two Miles. We had discover'd by our former Experiments, that the only Caution now necessary was, that the Wires conducted upon dry Sticks should not touch the Ground, each other, or any Non-Electric in a considerable Degree in any Part of their Length: if they did not touch each other, the Returns of the Wire, be they ever so frequent, imported little, as the Wire had been found to conduct Electricity so much better than the Sticks. It was therefore thought proper to place these Sticks in a Field fifty Yards distant from the Machine. The Length of this Field being eleven Chains or 726 Feet, eight Returns of the Wire from the Top to the Bottom of the Field made somewhat more than a Mile. and sixteen Returns more than two Miles, the Quantity of Wire intended for the Electricity to pass through to make the Experiment.

We had found last Year, * that, upon discharging the electrified Phial, if two Observers made their Bodies Part of the Circuit, one of which grasped the leaden Coating of the Phial in one Hand, and held in his other one Extremity of the conducting Wire; and if the other Observer held the other Extremity
of

of the conducting Wire in one Hand, and took in his other the short iron Rod with which the Explosion was made; upon this Explosion, I say, they were both shocked in the same Instant, which was that of the Explosion of the Phial. If therefore an Observer, making his Body Part of the Circuit, was shocked in the Instant of the Explosion of the charged Phial in the middle of the Wire, no Doubt would remain of the Velocity of Electricity being instantaneous through the Length of that whole Wire. But if, on the contrary, the Time between making the Explosion, and seeing the Convulsions in the Arms of the Observer holding the conducting Wires, was great enough to be measured, we then should be able to ascertain its Velocity to the Distance equal to half the Quantity of Wire employed only, let the Manner of the Electricity's discharging itself be what it would.

It has been a Question with some, who have consider'd this Subject, whether the Electricity, in completing the Circuit from the Matter contained in the Glass, pass'd, either by the Wire in the Mouth to the Coating of the Glass, the contrary Way by the Coating to the Wire in the Mouth, or otherwise directed itself both Ways at once? That the Electricity must pass off one of these three Ways was certain, as the Explosion would not be complete, unless in the Instant thereof some Matter very non-electric communicated between the Wire in the Mouth, and the Coating of the Glass. Unless therefore the Observer was placed in the Centre of the conducting Wires, it might be objected, that the Experiment was not made with the Exactness necessary; because any Person, who was of Opinion, that the Electricity
directed

Yards for their Turns round the Sticks, amounted to two Miles a Quarter and six Chains, or 12276 Feet.

As the Night preceding these Experiments had been very rainy, Care was taken, by silk Lines properly disposed, that the Wires in their Passage from the Window of the House might not touch the Wood thereof; lest, from the Moisture of this Wood, the electrical Circuit might be shortened.

When all Parts of the *Apparatus* were properly disposed, several Explosions of the charged Phial were made; and it was invariably seen, that the Observer holding in each Hand one of the Extremities of these Wires was convulsed in both his Arms in the Instant of making the Explosions.

Instead of one, four Men were then placed holding each other by the Hand near the Machine, the first of which held in his right Hand one Extremity of the Wire, and the last Man the other in his left. They were all seen convulsed in the Instant of the the Explosion. Every one who felt it, complained of the Severity of the Shock.

It was then desired by one of the Gentlemen concerned, that an Explosion should be made with the Observer holding only one of the Wires. This was done accordingly; but the Observer felt nothing, the Phial discharging itself in a different manner to what it did before, on account of the Circuit's not being completed.

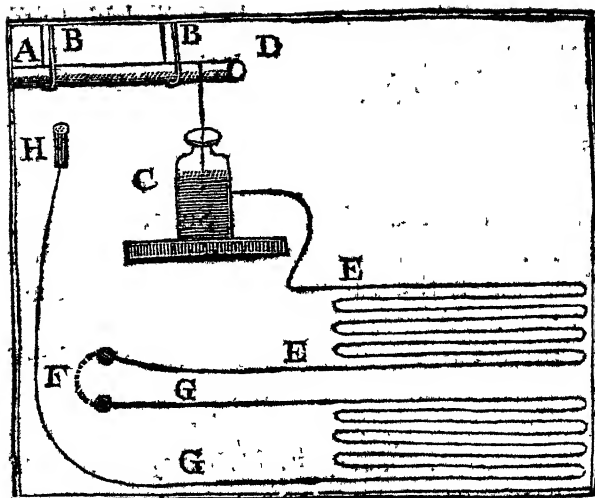
It was then tried, whether an Observer would be shocked upon the Discharge of the Phial, if the two Wires at their Extremities slightly touched each other, whilst an Observer at the same time held one of these about a Foot from their Ends in each of his Hands? Upon Trial he felt nothing, though the
Phial

Phial exploded very quick, because the iron Wire conducted the Electricity better than the Body of the Observer.

It was then tried, whether or no, as the Ground was wet, if the Explosion was made with the Observer holding the Extremity of each Wire standing upon the Ground near the Window of the House, any Difference would arise in the Success of the Experiment? No Difference was found, the Observer being shocked in the Instant of the Explosion as before in both his Arms, and across his Breast.

Upon these Considerations we were fully satisfied, that through the whole Length of this Wire, being as I mentioned before, twelve thousand two hundred and seventy-six Feet, the Velocity of Electricity was instantaneous.

As it was found last Year, we observed again, that although the electrical Commotions were very severe to those who held the Wires, the Report of the Explosion at the prime Conductor was little, in comparison of that which is heard when the Circuit is short. From whence it was conjectured, that the very loud Report, in the Experiment of *Leyden* is confined to a very short Circuit.



A, The prime Conductor.

B B, The silk Lines.

C, The coated Phial.

D, Its Hook communicating with the prime Conductor.

EE, The Wire reaching from the Coating of the Phial to the left Hand of the Observer, being more than a Mile in Length.

F, The Place of the Observer.

FF, A supposed Line, drawn upon the Explosion through his Body and Arms.

GG, Another Wire, of the Length of *EE*, which goes from the right Hand of the Observer to *H*.

H, The short iron Rod to make the Explosion.

2. *Some further Inquiries into the Nature and Properties of Electricity*; by William Watson F. R. S.

Read Jan. 21.
1747-8.

§ I. I PRESENTED to the *Royal Society* October 29. last a Paper containing some Accounts of what had been done by some Gentlemen of the *Society*, in order to examine, not only to what Distance the electrical Power was perceptible, but also to investigate, as near as might be, the respective Velocities of Electricity and Sound. Electricity indeed is the Subject of the present Paper, yet, as it relates to *Phænomena* thereof different from those mention'd in the former, I thought proper to separate them.

§ II. I took notice, in my Sequel to the Experiments relating to Electricity *, of an Observation of the ingenious Professor *Bose* of *Wittemberg*, viz. ' that if the electrifying Machine is placed upon ' Originally-Electrics, the Man who rubs the Globe ' with his Hands, even under these apparently favourable Circumstances, gives no Sign of being electrified when touched by an unexcited Non-electric. ' But if another Person, standing upon the Floor, ' does but touch the Globe in Motion with the End ' of one of his Fingers, or any other Non-electric, ' the Person rubbing is instantly electrified, and that ' very strongly.' This Experiment, almost a Year since, Dr. *Bevis* carried further, by placing what-

ever Non-electric touched the Globe as a Conductor, whether it were a Man or a Gun-barrel, upon Originally-Electrics. If then, either the Man who rubbed the Globe, or he who only held his Finger near the Equator thereof, were touched by any Person standing upon the Floor, a Snapping from either of them, I say, was perceptible upon that Touch.

§ III. As in my Sequel I had asserted, and by many Experiments therein had endeavoured to evince, that, contrary to the received Opinion, the Electricity was not derived from Glass, the Air, or other Electrics *per se*, I was desired to consider how far this Experiment did not prove the reverse of that Assertion; inasmuch as neither the Man who rubbed the Globe, or he who touched it with his Finger, from their being here both supported by Originally-Electrics, could receive any Supply from the Floor; and yet both of them snapped upon the Touch of a Person not supported by Electrics *per se*. Many Experiments had proved that the Electricity was not derived from the Glass; and therefore it was concluded, by Dr. Bevis, and several others to whom this Gentleman shewed the Experiment, that the Electricity here was communicated to the Person rubbing from the Air, by means either of the suspended Gun-barrel, or of the Man who touched the Globe.

§ IV. I was by no means satisfied with this Conclusion, as being directly contrary to numberless Facts. From a careful Consideration therefore of the Experiment itself, from comparing its Effects with those of several others, and, in general, from surveying

ing all the Properties of Electricity we are hitherto acquainted with, I gave the following as my Opinion.

1. That what we call Electricity is the Effect of a very subtil and elastic Fluid, diffused throughout all Bodies in Contact with the terraqueous Globe (those Substances hitherto termed Electrics *per se* probably excepted), and every-where, in its natural State of the same Degree of Density.
2. That this Fluid manifests itself only, when Bodies capable of receiving more thereof than their natural Quantity are properly disposed for that Purpose; and that then, by certain known Operations, its Effects shew themselves by attracting and repelling light Substances, by a snapping Noise, Sparks of Fire, &c. directed towards other Bodies, having only their natural Quantity, or, at least, a Quantity less than those Bodies from which these Snappings, &c. proceed.
3. That no Snapping is observed in bringing any two Bodies near each other, in which the Electricity is of the same Density, but only in those Bodies in which the Density of this Fluid is unequal.
4. That this Snapping is greater or less, in proportion to the different Densities of the Electricity in Bodies brought near each other, and by which Snapping each of them becomes of the same Standard.
5. That Glass, and other Bodies, which we call Electrics *per se*, have the Property of taking this Fluid from one Body, and conveying it to another, and that in a Quantity sufficient to be obvious to all our Senses.
6. That, in the Experiment in Question, the Reason why no Snapping is observed by a Person upon

on the Floor touching him who rubs the Globe with his Hands standing upon Wax, without at the same time some other Non-electric supported by Originally-Electrics, or otherwise being in Contact with the Globe, is owing to whatever Part of this Man's natural Quantity of Electricity, taken from himself by the Globe in Motion, being restored to him again by the Globe in its Revolutions; there not being any other Non-electric near enough to communicate the Electricity to; and that therefore, in this Situation, the Electricity of this Man suffers no Diminution of its Density.

7. That the Fact is otherwise; when every thing else being as before, either a Gun-barrel suspended in silk Lines, or a Man supported by Wax, or such-like, is placed near the Globe in Motion; because then, whatever Part of the Electricity of the Person rubbing is taken from him, is communicated either to the other Man or to the Gun-barrel, these, from their Situation, being the first Non-electrics; to which the Electricity taken from the Person rubbing can be communicated.
8. That, under these Circumstances, as much Electricity as is taken from the Person rubbing, is given to the other; by which means the Electricity of the first Man is more rare than it naturally was, and that of the last more dense.
9. That the Electricity in either of these Persons is in a very different State of Density from what it naturally was, or from that of any Person standing upon the Earth; this last being in a middle State between the two other Persons; that is, he has not his Electricity so rare as the Man rubbing

bing the Globe, nor so dense as that of him supported by Electrics *per se*, and touching the Equator of the Globe.

10. That therefore the same Effect, a Snapping, is observed, upon bringing any Non-electric near either of these Persons, from very different Causes : For it is apprehended, that, by bringing the Non-electric near him, whose Electricity is more rare, this Snapping restores to him what he had lost ; and that, by bringing it near him, whose Electricity is more dense, it takes of his Surcharge, by which means their original Quantity is restored to each.

§ V. This Solution of this *Phænomenon*, without allowing any Part of the Electricity of either of these two Persons to be furnished by the circumambient Air, was satisfactory, not only to the Gentleman who proposed it, but to many of the *Royal Society*, excellent Judges of this Matter, to whom I shewed the Experiment : And this the more so, as it is to be observed, that if, under the before-mention'd Circumstances, the Person rubbing the Globe was touched by him who held his Finger to the Globe, the Snapping was much greater than if either of them touched a Person standing upon the Floor ; as the Density of the Electricity between these two Persons was so much more different than that of either of them to him on the Floor : Whereas did their Electricity proceed from the Air, from their being both electrified they ought not to snap at all from their touching each other ; or, admitting they did touch each other, they both of them, upon a Supposition that they did receive their Electricity alike from the Air, should manifest the same Accumulation Effect, and snap upon

upon the Touch of a Man standing upon the Floor, the contrary of which invariably happens.

§ VI. At this time I am the more particular concerning the Solution of this singular Appearance, as Mr. *Collinson*, a worthy Member of this *Society*, has received a Paper concerning Electricity from an ingenious Gentleman, Mr. *Franklin*, a Friend of his in *Pennsylvania*. This Paper, dated *June 1. 1747*. I very lately perused, by Favour of our most worthy *President*. Among other curious Remarks there is a like Solution of this Fact; for though this Gentleman's Experiment was made with a Tube instead of a Globe, the Difference is no-ways material. As this Experiment was made, and the Solution thereof given upon the other Side of the *Atlantic Ocean* before this Gentleman could possibly be acquainted with our having observed the same Fact here, and as he seems very conversant in this Part of Natural Philosophy, I take the Liberty of laying before you his own Words.

‘ 1. A Person standing on Wax, and rubbing a
 ‘ Tube, and another Person on Wax drawing the
 ‘ Fire; they will both of them, provided they do
 ‘ not stand so as to touch one another, appear to
 ‘ be electrified to a Person standing on the Floor;
 ‘ that is, he will perceive a Spark on approaching
 ‘ each of them with his Knuckle.

‘ 2. But if the Persons on Wax touch one another during the exciting of the Tube, neither of them will appear to be electrified.

‘ 3. If they touch one another after the exciting the Tube and drawing the Fire as aforesaid, there will be a stronger Spark between them, than was between either of them and the Person on the Floor.

‘ 4. After such a strong Spark neither of them discover any Electricity.

‘ These Appearances we attempt to account for thus:

‘ We suppose, as aforesaid, that electrical Fire is
 ‘ a common Element, of which every one of
 ‘ these three Persons has his equal Share before
 ‘ any Operation is begun with the Tube. *A*;
 ‘ who stands upon Wax, and rubs the Tube,
 ‘ collects the electrical Fire from himself into the
 ‘ Glass; and his Communication with the com-
 ‘ mon Stock being cut off by the Wax, his Body
 ‘ is not again immediately supplied. *B*, who
 ‘ stands upon Wax likewise, passing his Knuckle
 ‘ along near the Tube, receives the Fire which
 ‘ was collected by the Glass from *A*; and his
 ‘ Communication with the common Stock being
 ‘ cut off, he retains the additional Quantity re-
 ‘ ceived. To *C* standing on the Floor, both ap-
 ‘ pear to be electrified: For he, having only the
 ‘ middle Quantity of electrical Fire, receives a
 ‘ Spark upon approaching *B*, who has an over
 ‘ Quantity, but gives one to *A*, who has an un-
 ‘ der Quantity. If *A* and *B* approach to touch
 ‘ each other, the Spark is stronger; because the
 ‘ Difference between them is greater. After such
 ‘ Touch, there is no Spark between either of them
 ‘ and *C*, because the electrical Fire in all is re-
 ‘ duced to the original Equality. If they touch
 ‘ while electrifying, the Equality is never destroyed,
 ‘ the Fire only circulating. Hence have arisen
 ‘ some new Terms among us. We say, *B* (and
 ‘ Bodies alike circumstanced) is electrified posi-

tively; *A*, negatively; or, rather, *B* is electrified
 ' plus, *A*, minus. And we daily in our Experi-
 ' ments electrify plus or minus, as we think pro-
 ' per. To electrify plus or minus, no more needs
 ' be known than this; that the Parts of the Tube
 ' or Sphere that are rubbed, do in the Instant of
 ' the Friction attract the electrical Fire, and
 ' therefore take it from the Thing rubbing. The
 ' same Parts immediately, as the Friction upon
 ' them ceases, are disposed to give the Fire, they
 ' have received, to any body that has less. Thus
 ' you may circulate it, as Mr. *Watson* has shewn*;
 ' you may also accumulate or substract it upon or
 ' from any Body, as you connect that Body with the
 ' Rubber, or with the Receiver, the Communi-
 ' cation with the common Stock being cut off.'

The Solution of this Gentleman, in relation to
 this *Phænomenon*, so exactly corresponds with that
 which I offer'd very early last Spring, that I could not
 help communicating it.

§ VII. In Sect. 51. and 62. of my Sequel to the elec-
 trical Experiments, which I presented to the *Royal*
Society, last Year, from not having consider'd this Ex-
 periment in a statical View, and from not then ima-
 gining the Velocity of Electricity so great as we since
 have found it, I concluded, that the Snapping observed,
 if a Person standing upon the Floor touched the Man
 standing upon Wax, who turned the Wheel of the elec-
 trifying Machine placed likewise upon Wax, to be
 owing to the Inversion of the usual Course of the Elec-
 tricity; as that Snapping was only constant, when
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* See my Sequel, p. 64.

the Gun-barrel suspended in silk Lines was touched by Non-electrics. As from divers Experiments I had found that Electricity was not furnished by dry Air, by many more that it could not come down clean silk Lines; and as, from his Snapping, the Man upon the Wax argued the Presence of Electricity, I conceived that this could happen no other Way, than that the rubbing of the Globe by a Cushion or the Hand of a Man, gave it a Fitness to take off the Electricity, furnished by the suspended Gun-barrel from the Non-electric upon the Floor, and lodge it upon the Machine, and upon the Man who turned the Wheel thereof. But the Experiment of circulating the electrical Fire*, where the Brush of blue Flame from a blunt Wire properly disposed, can always be seen to pass diverging into the Machine, though not so, when brought near the Gun-barrel under the most favourable Circumstances; as well as the Experiment before-mention'd brought to shew that the Electricity came from the Air, have induced me to change my Opinion; and instead of the Course of the Electricity being inverted, the *Phænomena* arose, as far as I am capable of judging, from the Man who turned the Wheel of the electrifying Machine having less than his original Quantity of Electricity, and the Gun-barrel from having more: To these add, that the Person, who touched these while standing upon the Floor, had a Quantity different from each of these, that is, his natural Quantity.

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§ VIII. I beg Leave to correct also what I mention'd in my *Sequel*, p. 69. in relation to my suggesting, that, in the Explosion of the charged Phial through the Body of a Man, or other Non-electrics, as much Electricity as was taken from his Body, was immediately replaced by the Floor of the Room upon which he stood: I having since found, that the charged Phial would explode with equal Violence, if the Hook of the Wire, which is usually run through the Cork of the Phial, was bent in such a manner as to come near the Coating of the Phial, without any other Non-electric being near, from which such Quantity could be supplied.

§ IX. I take notice of these, inasmuch as, notwithstanding the very great Progress that has been made in our Improvements in this Part of Natural Philosophy within these few Years, Posterity will regard us only as in our Noviciate; and therefore it behoves us, as often as we can be justified therein by Experiment, to correct any Conclusions we may have drawn, if others yet more probable present themselves.

§ X. I laid down and consider'd largely in my *Sequel* *, that the Stroke from the Phial, in the Experiment of *Leyden*, was not in Proportion to the Quantity of Matter contained in the Glass, but was increased by the Quantity of Matter in the Glass, and the Number of Points of non-electrical Contact on the Outside of the Glass. This Fact I have pursued further, and increased thereby the electrical Explosion

* See p. 11, 17, 19, &c.

sion to an astonishing Degree. To this End I procured three cylindrical Phials blown very thin, about seventeen Inches in Height and four in Diameter: After these were coated within an Inch of their Necks with Sheet-Lead, I put into each fifty Pounds of leaden Shot. I chose this Form for the Glasses, that the Matter therein contained might be exposed under as large a Surface, as could conveniently be obtained. These Glasses were placed near each other in a convenient Part of my Room, and did communicate with each other by means of a small iron Rod lying upon all their Mouths, and touching Pieces of strong Wire stuck into the Shot contained in them: By this Management one of these could not be electrified without communicating with the rest. The leaden Coatings of these Glasses were also connected together by small Wires, all which center'd in one tail Wire; so that, when the Matter contained in these three Glasses was replete with Electricity, which was done by a Wire from the Gun-barrel fastened to the iron Rod lying upon their Mouths, the whole Quantity of Electricity here accumulated might be discharged at once by touching the Gun-barrel with an iron Rod fastened to the tail Wire. When the Glasses are sufficiently electrified, if the Room is dark, you will see Brushes of blue Flame from several Parts of the conducting Wire; and these indicate the proper Time of making the Explosion. These Glasses, from the Thinness of their Sides, and from the Weight of their leaden Shot, are very liable to burst; and if one of them happens to have the least Crack in any Part of its Surface, which is under the Lead, none of them can be electrified; all the Electricity
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passing off by that Crack. The electrical Explosion from two or three of these Glasses is not double or treble to that from one of them; but the Explosion from three is much louder than that from two, that from two much louder than that from one.

§ XI. The Experiment just mentioned induced me to imagine, that the Explosion from these Phials was owing to the great Quantity of non-electric Matter contained in them: And whilst I was considering of some certain Method of assuring myself whether the Fact were so, Dr. *Bevis* informed me, that he had found the electrical Explosion to be as great, as when he had accumulated the Electricity in a half Pint Phial of Water, by the following Method. He covered a thin Plate of Glass, of about a Foot square on both Sides, with Leaf-Silver; this he made to adhere to the Glass with very thin Paste. A Margin of an Inch was left on both Sides; otherwise, upon electrifying this Plate, the Electricity would be prevented from being accumulated upon one of its Surfaces, by being propagated from the Silver on one-Side to that of the other. When the glass Plate was thus prepar'd, if it was placed upon a Table in such a Manner, that, when fully electrified by a Wire or such-like from the prime Conductor, a Person touched the under Surface with a Finger of one of his Hands, and brought one of the Fingers of his other near the upper Surface thereof, or near the prime Conductor, he was shocked in both his Arms and across his Breast. The same Effect happen'd, if, when this Plate was electrified in the before-mention'd Manner, a Person holding it in his Hand by the Margin, and without touching the Silver,

Silver presented it, even some time after it had been taken from the prime Conductor, to another Person who touched the under Surface with his Finger, and held it there till he touched the upper Surface with a Finger of his other Hand.

§ XII. This Experiment was sufficiently convincing, that the Greatness of the electrical Explosion, in my former Trials, was not owing solely to the great Quantity of non-electric Matter contained in the Glasses ; as the Explosion from the glass Plate silvered was occasioned by about six Grains of Silver, upon which the Electricity was accumulated ; more especially as this Explosion was equal, if not superior, to that from half a Pint of Water contained in a thin Glass as usual, under the most favourable Circumstances.

§ XIII. As each of the Surfaces of the glass Plate just mentioned measured sixty-four square Inches, I was desirous of pursuing this Inquiry further ; and accordingly procured a cylindrical glass Jar blown very thin, of sixteen Inches in Height, and eighteen Inches in Circumference. This I caused to be covered both within and without with Leaf-Silver, to within an Inch of its Top. This Glass with its Margin made very clean (upon which the Success of the Experiment considerably depends) was fully electrified by the means of a Piece of Chain, let down to the Bottom of the Jar, by a Wire from the prime Conductor ; and the Explosion made by its being placed upon a Plate of Metal, to which was fasten'd a Wire connected to an iron Rod, and this Rod was

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brought near some gilded Leather lying upon the prime Conductor. This Explosion was equal to that from the three Glasses before-mention'd, containing a hundred and fifty Pounds of leaden Shot; though here the Weight of the Silver lining the internal Surface of the Glass, upon which the Electricity was accumulated, did not exceed thirty Grains. So much of the internal Surface of this Jar, as was covered with Silver, amounted, as the Surfaces of Cylinders are as their Length multiplied by their Periphery, and allowing thirty six square Inches for the Bottom, to three hundred and six square Inches. If this Explosion was made in a dark Room, the Corruscations within the Jar, at the Instant of the Explosion, were extremely brilliant.

When this Jar is fully electrified, if, instead of making it explode, you only bring the short iron Rod, with which the Explosion is usually made, near a Piece of gilded Leather lying upon the prime Conductor, though not near enough to make the Glass explode at once, you hear the Electricity, accumulated within the Jar, escape with a Noise very like that of a small heated iron Bar quenching in Water.

§ XIV. The great Explosion from the Jar before-mention'd, when so little non-electric Matter was included therein, has caused me to be of Opinion, that the Effect of what we call the Experiment of *Leyden* is greatly increased, if not principally owing, not so much to the Quantity of non-electrical Matter contained in the Glass, as to the Number of Points
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of non-electrical Contact * within the Glass, and the Density † of the Matter constituting those Points, provided this Matter be in its own Nature a ready Conductor of Electricity. For this Reason it is presumed, that so much of the Lead contained in the Shot in the before-mentioned Experiment, only concurred to make the electrical Explosion, as touched the internal Surface of

* Bodies having the Power of readily conducting Electricity seems to depend very little upon their specific Gravity simply considered: Metals, for Instance, and Water, are in a great Degree Non-electrics, and consequently conduct Electricity the best of any Substances, that have yet fallen under our Notice; whereas the *Calxes* of Metals, though very dense Bodies, and very greatly more so than Water, prevent in a great Degree the quick Propagation of the electrical Power. So that a Phial coated within and without with Ceruse, *i. e.* the *Calx* of Lead, and electrified, did not, upon the Application as usual of one Hand to the external Surface thereof, and touching the prime Conductor with the other, occasion any Shock, or make any Explosion more than the simple Stroke from the prime Conductor. The same Observation holds good with regard to red Lead, Litharge, and lunar Caustic or the *Calx* of Silver, none of which snap, when electrified. For the same Reason, Filings of Iron, which are rusty, *i. e.* have their Surfaces converted into a *Calx*, are much less proper to be put in Glasses to make the Experiment of *Leyden*, than those that are not; inasmuch as these last cause a much louder Explosion than the first. The making use of rusty Filings of Iron was the Occasion of my mentioning in my *Sequel*, § XVI. that the Stroke from these was less than that from Water; the contrary of which I afterwards found true, when Filings of Iron not rusty were substituted.

† I heretofore, p. 11, &c. of my *Sequel*, took notice, how much the Effect of this Experiment depended upon the Quantity of non-electric Contact upon the Outside of the Glass.

of the Glafs: As a great Part of this Surface was without Contact, occasioned by such of the Shot as presented themselves thereto, touching, from their spherical Figure, only in one Point, there consequently remained without Contact comparatively great Spaces between each Shot. This Defect was obviated by the universal Contact of the Silver, and thereby was occasion'd the greater Explosion.

§ XV. The following Experiment has some Relation to the preceding. If a Phial of warm Water, without being coated with Sheet-Lead, or other non-electrical Matter, is electrified by connecting it to the prime Conductor; and a Ring of small Wire, in lieu of the usual Coating, is put round this Phial, the Wire being continued of a sufficient Length to touch the prime Conductor; upon discharging the Phial, you have a slight Explosion, and a Flash of Fire seems at that Instant to fill the Glafs. But if this Experiment is made in a very dark Room, and with great Attention, this Flash in the Phial will not then seem to proceed from the whole Quantity of Water contained therein; but, as far as the Suddenness of the Explosion will permit the Eye to follow it, will be seen to occupy only the internal Surface of the Phial.

§ XVI. I order'd another glafs Jar as large as possible to be blown, so that the Glafs thereof might be very thin; and after many Attempts of the Glasmakers I procured one, the Height of which was twenty two Inches, the Periphery forty-one. This was covered within and without, leaving a Margin
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of an Inch at Top, with Leaf-Brass. As much of the internal Surface as was covered amounted to 1129 square Inches. But the Difficulty I met with in procuring this Glass was sufficiently recompensed by the great Increase of the Explosion therefrom, when fully electrified, and discharged in the same manner as the glass Jar before-mention'd. The Report was vastly louder; all the attendant *Phænomena* greatly exceeded any thing of this kind I was before acquainted with. As the Quantity of Metal within this Jar did not exceed two Drams, this Experiment gives further Weight to my Opinion before-mention'd § XIV. in relation to the manner of increasing the Effects of the Experiment of *Leyden*; and from what the *Phænomena* of that surprising Experiment principally proceed; *viz.* not from the Volume of the prime Conductor, nor from the Quantity of non electrical Matter contained in the Glass, but from the Number of Points of non-electrical Contact both within and withoutside of the Glass, and from the * Density of the Matter constituting those Points.

§ XVII. It must be observed, that, *cæteris paribus*, the electrical Explosion is greater from hot Water included in Glasses than from cold; and from these glass Jars warmed than when they are cold.

§ XVII.

* Though the Density of the Matter constituting these Points proceeds from their Number in a mathematical Sense, yet in a popular one I take the Liberty to distinguish them.

§ XVIII. The Explosions from the large Glasses just mention'd fully electrified, as well as from small ones under the same Circumstances, will not be considerable, unless the Circuit, frequently mention'd in my Writings upon this Subject, be completed; that is, unless some Matter, non-electric in a considerable Degree, and in Contact with the Coatings of the Phials, is brought into Contact, or nearly so, with such Non-electrics as communicate with the Matter contained in the Phials themselves. When indeed the Circuit can be completed, the Explosion from the large Glasses is prodigious; the whole Quantity of Electricity therein accumulated, or nearly so, being discharged in an Instant. But the Fact is otherwise, if the Circuit is not completed, and the iron Rod in the Mouth of one of these Phials is touched by a Non-electric (the Hand of a Man, for Instance) not in Contact with the tail Wire: For then there will be no Explosion, no Shock; but the Person, approaching his Finger near the iron Rod, will see a Succession of small Sparks, more intensely red than that large one seen, when the Phials explode at once; and the Person making the Experiment, will feel a very pungent Pain, but confined to that Finger which touches the iron Rod. This Succession of Sparks continues, until the Electricity accumulated in the Phials is nearly exhausted. So that the Explosion from any given Quantity of Electricity, accumulated as before-mention'd, is greater or less in proportion to the Time expended in making that Explosion: In like manner as a given Quantity

tity of grained Gunpowder rammed hard in a Pistol, is almost instantaneously fired, and that with a great Report; when the same Quantity of Gunpowder rubbed fine, and rammed hard, takes a considerable Time in burning as a Squib, and makes no Explosion.

§ XIX. The Causes why the charged Phial will not explode quick, without the Electricity therein describing a Circuit through Substances non electric in a great Degree, may be very difficult to be assigned. It is sufficient for us in the present Inquiry to be assured of its being a certain, an invariable Law: And in order to prove, that the Electricity, upon the Explosion, passes with its whole Force through the Circuit of Non-electrics, contrary to what has been suggested, I made the following Experiment.

§ XX. I procured two small square iron Bars, of about fourteen Inches long: An Inch at each End of these I caused to be bent at right Angles. These iron Bars were supported in such manner (by Substances whether originally electric, or not, was no ways material) that each of their Ends came within about two Tenths of an Inch of some warm Spirit of Wine, or Essence of Lemons in four Spoons placed upon a Table. I then suspended a common coated Phial filled with Filings of Iron to the Gun-barrel, the tail Wire of which reached to a Table at a few Feet Distance, and was placed under a brass Weight which supported the Handle of the first of the Spoons: Over this Spoon, at the Distance just mention'd, I placed one of the square iron

iron Bars, and at its other End was placed another Spoon: This second Spoon touched the Handle of the third, which was placed under one End of the other square Bar, whose other End came near to the Spirit in the fourth Spoon, the Handle of which lay upon a Weight; and under this was placed a Wire connected to the short iron Rod, with which the Explosion was made, when the coated Phial was charged. When the Phial was well charged, if the Spirit of Wine sent forth Vapours, and the square iron Bars were at a proper Distance from it; upon making the Explosion at the Gun-barrel the Electricity snapped between the Spirit and the iron Bars, and the Spirit was set on Fire at the same Instant in all the Spoons. It sometimes happened, that some of them only were fired. If the iron Bars were too near the Spirit, it was not fired, though the Circuit was completed; because then no electrical Flame snapped between the Rods and Spirit; that Effect happening only, when the Parts of the Non-electrics describing the Circuit are not in immediate Contact; on the other hand, if the Space left between the Bars and Spirit was too great, the Circuit could not be completed, and there would be no Explosion.

§ XXI. This Experiment will seem more surprising in the following manner. When the *Apparatus* is disposed of as before, the tail Wire from the coated Phial, before it reaches to the Table, is fasten'd to an iron Rod standing in a Pail of Water: Another iron Rod is likewise placed in the same Pail of Water, and a Wire from this last reaches under the Weight, which supports the first of the
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before-mention'd Spoons. From beneath the Weight which supports the Handle of the fourth Spoon, a Wire reaches to an iron Rod standing in a second Pail of Water, in which is placed also another iron Rod, to which is fastened another Wire connected with the short iron Rod, which is employed to make the Explosion. When, with this Disposition of the *Apparatus*, the charged Phial is caused to explode, the Spirit or Effence of Lemons in some or all of the Spoons is set on Fire; to accomplish which, the Electricity must necessarily pass through one of the Pails of Water, and possibly through both. But here it must be understood, that the Pails of Water stand upon a dry wooden Floor; for if they stand upon one that is wet, or upon the Ground, the Circuit will be, for Reasons frequently mention'd in the Course of these Inquiries, completed between the two Pails, where the non-electric Matter is continuous, and be prevented from passing by the Spoons where it is not so; and this will defeat the Success of the Experiment. The Number of Spoons in the Manner before-mention'd, and their Distance from each other, may be varied as far as is thought necessary. The Circuit may likewise be directed through any Number of Men, provided that each of them holds in one of his Hands a Spoonful of warm Spirit, and brings one of the Fingers of his other Hand at the proper Distance to the Spirit held in the Hand of the Person next him: By these means the Explosion of the charged Phial will set on Fire the Spirit in several of the Spoons at

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the same time, provided the Persons employ'd hold their Hands sufficiently steady.

§ XXII. This Experiment exhibits new and unexpected *Phænomena*: In all the Experiments to kindle inflammable Substances by Electricity hitherto attempted both here and abroad, either the Spirit or the Non-electric, wherewith it was intended to be set on Fire, were placed upon Originally-Electrics. But here, on the contrary, although both one and the other are placed upon Non-electrics, we see the same Effect produced. Nor is the electrical Power lessened, by exciting several different Quantities of Flame; in doing which, it passes so quick as to prevent the Possibility, in several Spoonfuls of Spirit, fired by the same Operation; of determining which of them was on Fire first: And though we know from its Effects, that the Electricity goes through the whole Circuit of Non-electrics with its whole Vigour, its Progress is so quick as not to affect, by attracting or otherwise, light Substances disposed very near the Non-electrics, through which it must necessarily pass.

§ XXIII. I would here recommend to those Gentlemen of the *Royal Society*, who last Summer measured the respective Velocities of Electricity and Sound, a Process of this sort to be executed at a proper Time; whereby they would be able to a very great Nicety to ascertain the absolute Velocity of Electricity. For it may be contrived, that a Man may be placed in the same Room with the electrifying Machine, taking hold of a Wire in each of his Hands:

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These Wires may be so managed, that by means of the electrical Circuit, the Man holding them may be made sensible of the electrical Commotion, even under the Eye of an Observer at the Machine; though before the Electricity can arrive at the Person holding the Wires, it will be obliged to pass through whatever large Space shall be thought convenient for the Observation. The Time then spent between the Explosion of the charged Phial, and the Person holding the Wires feeling the electrical Commotion, will give the absolute Velocity of Electricity to great Exactness*.

XXIV. As my Inquiries upon the Subject of Electricity have always tended as much as possible to the Analysis thereof, I have often observed, that if, when the electrifying Machine stands upon the Floor, the Globes thereof are rubbed with their Cushions, or with Hands cover'd with Originally-Electrics of a sufficient Thickness, and perfectly dry, no Electricity will be perceptible upon the Touch of a Gun-barrel suspended in silk Lines, and touching the Globe in Motion, or upon the Touch of any other Substances supported by Electrics *per se*; or, in other Words, there will be no Accumulation of Electricity. The only Originally-Electrics fit for this Experiment (as all unctuous Substances, as Wax, Resin, and such-like, though Electrics *per se*, by sticking to the Outside of the Glass render it unfit to excite Electricity

* This has been since put in Execution. See above p. 88.

tricity from other Bodies) are to be obtained from the Animal Kingdom : And of these only such as do not partake, from their Manufacture or otherwise, of any non-electric Substances. Those of this sort, which I have tried, and always with the same Success, when perfectly dry, have been Silk (woven or not), Velvet, Hair-Cloth, Woollen-Cloth, and the dry Skins of Rabbits dressed in their Fur ; and the Event has been the same, whether these Substances have been rubbed under a greater or a less Degree of Friction : And scarce any Electricity has been perceptible, when those Parts of these Substances, which immediately are in Contact with the Globes, have been rubbed over with dry Chalk, a non-electric Substance. But the Success is different, when these originally-electric Substances have lain in damp Places, or have been held over the Steam of warm Water ; because then the Water imbibed by these Substances serves as a Canal of Communication to the Electricity between the Hands or Cushions and the Globes in the same manner, as the Air, replete with Vapours in damp Weather, prevents the Accumulation of Electricity in any considerable Degree, by conducting it as fast as excited to the nearest Non-electrics. On the contrary, most Substances of the Vegetable Kingdom, whose Form makes them fit for this Treatment, though made as dry as possible, furnish Electricity, though in different Quantities. I have tried Hemp, Linen-Cloth of various Kinds, Paper both of Linen and Hemp, Cotton in the Wool, Fustian, Cotton-Velvet, and

and many others of this Class. I have covered at one time the Cushion, with which I rubbed a Globe, with eight Lamina of Sheet-Lead, and have excited Electricity from that Metal: And however improper a Deal-Board may seem for the Purpose of rubbing a Globe, I have more than once accumulated Electricity from that, though its Substance has the Appearance of being much less fit than every one of the Originally-Electrics I mention'd before.

§ XXV. To the Doctrine here laid down it may be objected, that Leather is an animal Substance, which, though perfectly dry, excites Electricity the strongest of all the Substances hitherto discover'd; that dry Leather ought to be consider'd as an Originally-Electric; and therefore, according to the Rule before-mention'd, should not furnish, from rubbing the Globe therewith, any Electricity at all. To this I answer, that though the dry Skins of Animals are Electrics *per se*, dry Leather is far from being so; and this is owing to the vast Quantities of restraining vegetable Substances imbibed by the Skins throughout their whole Contexture in the Operation of Tanning in some Species of Leather, and of saline Substances, such as Alum; in others; both which Substances are non-electric, and of these Leather very considerably partakes: For, by these the Hides and Skins of Animals (and any Muscle of their Bodies is liable to the same Treatment), which otherwise are as putrescent as any Part of their Bodies soever, are made to last through many Ages, and be subservient to many

many valuable Purposes of Life. The same Conclusion must be drawn concerning Hats, which, tho' made of the Hair of Animals, furnish Electricity, though but in a small Degree: And this is occasion'd by the mucilaginous and gummy Substances made use of by the Hatmakers, to give their Manufacture a suitable Stiffness.

§ XXVI. From what I have advanced § XI. XII. XIII. XIV. XV. XVII. it may possibly be conjectur'd, that the electrical *Effluvia* occupy only the Surfaces of Bodies electrified; as we there found, that a very small Quantity of Matter, distributed under a very large Surface, would occasion a greater Accumulation of Electricity, than a very much more considerable Quantity of Matter under a less. But that the Electricity occupies the whole Masses of Bodies electrified, and passes through their constituent Parts, is clearly demonstrated by the following Experiments.

§ XXVII. When I first engaged in these Inquiries, to assure myself of this Fact, I enveloped an iron Rod about three Feet in Length with a Mixture of Wax and Resin, leaving free from this Mixture only one Inch at each End. This Iron was warmed, when thus fitted, that the whole of its Surface, where it was intended, might be covered. This Rod, when electrified at one of its Ends, snapped as strongly at the other, as though it was without the Wax and Resin. This could not have happened from the Electricity's passing along the Surface of the iron Rod, because there it was prevented
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by the Originally-Electrics, and consequently must of Necessity pass through it.

§ XXVIII. A Phial of Water, in the Experiment of *Leyden* can be electrified, and may be caused to explode, though the Wire, touching the Water in the Phial in making that Experiment, be run through a wax Stopple, exactly fitted to the Mouth of the Phial.

§ XXIX. I caused a glass Tube, open' at each End, and about two Feet and a half long, to be capped with Brass cemented to the Ends of the Tube. In the Centre of each of these Caps was fastened a slender brass Rod; and these were disposed so in the Tube as to come within half an Inch of each other. When the Tube was properly suspended in silk Lines with one of its Extremities near a glass Globe in Motion, the brass Work at both Ends snapped equally strong. As the Electricity could not pass along the Surface of this Tube warmed and wiped clean, this Effect could not have happened, unless the Electricity pervaded the Substance of the brass Caps. Upon touching the Brass at the End of the Tube most remote from the electrifying Machine, the Snaps from one of the brass Rods within the Tube to the other were seen to correspond with the Snaps without. More Experiments of this kind might be added, but these, I presume, are sufficient to shew, that the Electricity occupies the whole Masses of non-electric Bodies electrified. That the Electricity passes through Originally-Electrics to a certain Thickness I took notice of in a Paper I did myself the Honour to communicate in *February* 1745.

§ XXX.

§ XXX. I shall forbear at present to lay before you a Series of Experiments *in vacuo*; from the Comparison of which, with the Experiments in open Air it appears, that our Atmosphere, when dry, is the Agent, whereby, with the Assistance of other Electrics *per se*, we are enabled to accumulate Electricity in and upon Non electrics; that is, to communicate to them a greater Quantity of Electricity than they naturally have: From hence also we shall see, that, upon the Removal of the Air, the Electricity pervades the *Vacuum* to a considerable Distance, and manifests its Effects upon any Non-electrics, which terminate that *Vacuum*: And by these Means that originally-electric Bodies, even in their most perfect State, put on the Appearance of Non electrics, by becoming the Conductors of Electricity. But these Matters may possibly be the Subject of a future Communication.

IV. *A Letter from Mr. Francis Drake, Surgeon, F. R. S. to Martin Folkes Esq; President of the Royal Society, &c. concerning the Bones of a Fœtus being discharged thro' an Ulcer near the Navel.*

Worthy Sir, York, June 22. 1747.

Read Jan. 28. 1747. **H**AVING a Call from hence into *Lincolnshire* lately, to see a Patient, the Apothecary who attended on him informed me, amongst other things, of an extraordinary Case, which had happened in that Neighbourhood, a very few Years ago. I have since been informed, on Inquiry, that it has not as yet been represented to the *Royal Society*; and therefore I hope you will do me the Honour to lay this Account of the Case before them.

Jane, the Wife of *James Burman*, Labourer, at *Scawby* near *Brigg* in *Lincolnshire*, was about 29 Years of Age when she married. About two Years after, when she had had a Child at full Time, she conceived again, and went regularly on for four Months. She then got a Fall, and about three Weeks after felt a Load in her Belly, which continued, on the right Side of the same, for between two and three Years. The Woman then grew very big with another Child, which pressed so much upon the Lump as to give her great Uneasiness. However, she went on to her Time with her double Burden; and, three Years and a Quarter after the accidental Fall, she

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was deliver'd of a live Child at full Growth: From which Time she grew worfe and worfe, with violent Pain about the Navel, and an inflam'd Tumour appeared near that Part. Upon Application to a neighbouring Surgeon, Fomentations were used, which produced a Suppuration at a small Breach near the Navel. The Surgeon did not know what to make of this Swelling, and therefore did not venture to enlarge the Orifice; but it continued discharging a fetid purulent Matter for three or four Months longer.

About a Year, or more, after her last Delivery, the Woman was suddenly seized in the Night-time, and a hardish Mass of Flesh, seemingly about eight Inches long, was discharged thro' the old Opening in her Belly. The Lump was rather thicker than an ordinary Man's Wrist, and which, being opened, contained all the Bones of a *Fœtus*, of about four Months Growth.

At this time the Woman was much emaciated, occasioned by the large Discharge of *Pus* from the Wound; and, what was much more extraordinary, whatever she eat or drank, came half-digested thro' the Opening. White Bread, or better Diet, came thro' in that Manner; but coarse Rye-Bread, or such-like, was not digested at all. For which Reason, the poor Woman must have inevitably perished, had she not been supported by a charitable Gentleman's Family in the Village, with Diet fit for her miserable Circumstances.

She continued to discharge her Excrement in this Manner for six Months, and then that Symptom left her; after which the Ulcer was kept open other six Months,

Months, when it dried up of itself naturally, with a very firm but small Cicatrix.

I had the Curiosity to see this Woman, and Mr. *Charlesworth*, Surgeon and Apothecary at *Brigg*, sent for her. She appeared hale, strong, and in full Health. I had the above Account of her Case from her own Mouth, attested by the Surgeon who attended her. I saw the Bones of the *Fætus* in Mr. *Charlesworth's* Possession, perfectly white, and, I believe, not one wanting. The Woman further told me, that, nine Months after the Wound was healed, she was deliver'd of another live Child, at full Time, but with great-Difficulty. The whole Time that the Bones of the *Fætus* may be supposed to have lain in the Woman's Belly was about four Years and a-half.

Thus, Sir, I have drawn up the Account as well as I can, but very inaccurately. I have purposely omitted Terms of Art, in order to make myself better understood by those who are not Surgeons or Anatomists. There are several Particulars in the Account, which I cannot reconcile to any natural Laws that I am acquainted with: However, as the Truth of the Whole is incontestable, it shews most evidently what wonderful Things Nature can do, with proper Assistance.

I am, Sir, with my best Respects to the Gentlemen of the *Society*,

*Your most obliged, and
most obedient humble Servant,*

Francis Drake.

V. *An Account of the Giants Causeway in Ireland, in a Letter to the President from the Rev. Richard Pococke, LL.D. Archdeacon of Dublin, and F.R.S.*

Dublin, 5 Jan. 1747-8.

Read Jan. 28. — ^{1747.} **I**N my last Passage over to this King-

dom, I saw that very remarkable Curiosity, commonly called the Giants Causeway: § The Sea-Cliffs are very high thereabouts, and what is called the Causeway is a low Head, extending from the Foot of the Cliffs into the Sea like a Mole. This Head does not appear at first so grand as it is represented in the Views engraved of it; but when one comes to walk upon it, and consider it more attentively, it appears to be a stupendous Production of Nature. The Head ends in two Points: I measured the more Western to the Distance of 360 Feet from the Cliff, and it appeared to me to extend about 60 Feet further; but this Part I could not measure, by reason that the Sea was then high; and I was told, that at low Tides it might be seen about 60 Feet yet further upon a Descent losing itself in the Sea. I also measur'd the more Eastern Point 540 Feet from the Cliff, and saw about as much more of it as of the other, when it winds about to the Eastward, and is also lost in the Water. One may walk upon this Head on the Tops of the Pillars to the Edge of the Water. These Pillars are of all angular Shapes from three Sides to eight. The Eastern Point, towards that End where it joins the Rocks, terminates it self for some Way in a perpendicular

§ This Causeway is before taken notice of in N^o. 199, 212, 235, and 241 of these *Transactions*.

perpendicular Cliff, formed by the upright Sides of the Pillars, some of which I measured, and found to be 33 Feet and 4 Inches in Height. They say there are in all 74 different Sorts of Figures among them. Each Pillar consists of several Joints or Stones lying one upon another, from 6 Inches to about a Foot in Thickness: Some of these Joints are in the middle so convex, as for those Prominences to be nearly Quarters of Spheres, round each of which is a Ledge, upon which the Stones above them have rested, every Stone being concave on the under Side, and fitting in the exactest manner upon that which lies next below it. The Pillars are from one to two Feet in Diameter, and consist most commonly of about 40 Joints, most of which separate very easily, tho' some others, which are more strongly indented into each other, cohere strongly enough to bear the being taken away in Pairs.

But the Causeway is not I think the most singular Part of this extraordinary Curiosity; the Appearance of the Cliffs themselves being yet to me more surprising; these and their several *Strata* I examined from the Rocks on the other Side of a little Bay, about half a Mile to the East of the Causeway. I thence observed, that there runs all the Way a *Stratum* from the Bottom of black Stone, to the Height, as well as I could conjecture, of about 60 Feet, divided perpendicularly at unequal Distances by Stripes of a reddish Stone, looking like Cement, and about 4 or 5 Inches in Thickness. Upon this there is another *Stratum* of the same black Stone divided from it by a *Stratum* 5 Inches thick of the red. Over this another *Stratum* of Stone ten Feet thick divided in the same manner; then a *Stratum* of the red Stone twenty Feet deep; and above that a *Stratum*
of

of upright Pillars. Above these Pillars lies another *Stratum* of black Stone 20 Feet high; and above this is again another *Stratum* of upright Pillars rising in some Places to the Top of the Cliffs, in others not so high, and in others again above it, where they are called the Chimneys.

This Face of the Cliffs reaches for two computed Miles East from the Causeway, that is about 3 measured *English* Miles, to the House of Mr. *John Stewart* two Miles West of *Balintoy*. The upper Pillars seem to end over the Causeway, and, if I mistake not, become shorter and shorter as one goes from it, lying between two Binds of Stone like Seams of Coal, and like those little Pillars found in *Derbyshire* §.

These Binds probably meet together all round, and inclose this extraordinary Work of Nature; and if so, the Pillars must be very short towards the Extremities.

I was led to this Conjecture by the following Observations: The lower *Stratum* of Pillars is that which goes by a Descent into the Sea, and which makes what is called the Giants Causeway; and where this Descent approaches the Sea, it seems probable that the Pillars become shorter and shorter, so as to end not much further off. Now the upper Bind of this *Stratum* may have been of so soft a Nature, as by degrees, in Process of Time, to have been washed away by the Sea. And in the Cliff over the Causeway I saw several Pillars lying along in a rude manner almost horizontally, which seemed to me to be some of the Pillars of the upper *Stratum* fallen down by the giving way of the Bind which.

§ The Doctor here refers to a Paper of Mr. *Emanuel Da Costa*, communicated May 14. 1747. but not yet printed.

which was under them, and over the lower ones that compose the Causeway. And here most probably the upper Pillars ended, as they are seen no farther in the Cliff. I saw the Tops of Pillars even with the Shore, both on the the East and West Sides of the Causeway, and some much lower than the Causeway itself; and it is probable that these are much shorter than those of the Causeway, which I measured above thirty Feet higher than the Tops of them.

When I was upon the Causeway, I saw in the Cliff, to the South-east, what they call the Organs, about a Quarter of a Mile off, and a third Part of the Way up the Cliff. They appeared small, and somewhat like a black *Stalactites*: They were not commonly known to be such Pillars as the others; but they are so, and belong to the lower *Stratum*. When with great Difficulty I climbed up the steep Hill to them, I found they were hexagonal, and larger Pillars than most of the others, being about 2 Feet in Diameter; and I measured 5 Sides of one of them, which were of 13, 15, 12, 21, and 16 Inches respectively. The Joints I could come at were about 9 Inches thick, and each Pillar, as well as I could count, consisted of between 40 or 50 of them: These Joynts are almost flat and plain, the Convexities on their upper Faces being so small as to be scarce discernible. I enquired whether any of these Pillars were found in the Quarries within Land, and the People there told me they were not; but since I left the Place, I have been assured by others, that there are some found two or three Miles from the Shore. I am, with the greatest Regard, Sir,

Your most obedient humble Servant,

Richard Pococke.

VI. *A Letter from Maurice Johnson Esq; President of the Gentlemens Society at Spalding, to James Jurin M. D. Fellow of the Royal College of Physicians, London, and F. R. S. concerning a Metalline Thermometer, in the Museum of that Society.*

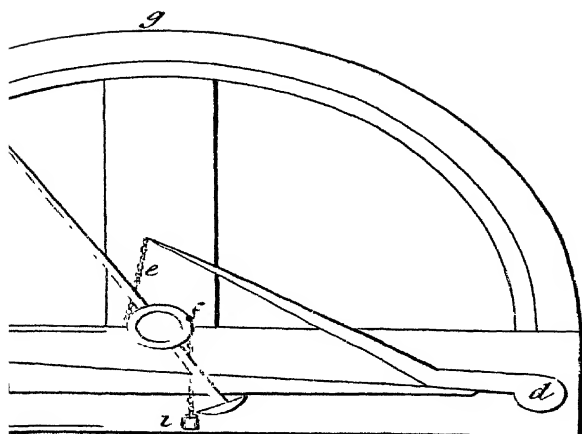
S I R,

Spalding, Jan. 16. 1747-8.

*Read Jan. 28.
1747.*

A S I know it must give you Pleasure, and, being by you (as I desire it may be at their next Meeting) communicated to the *Royal Society*, may be of some Credit to the Memory of the Inventor the late Mr. *Samuel Frotheringham*, a Grazier at *Holbeach* in *Elloe Holland, Lincolnshire*, and of some Profit to the Maker, give me Leave to acquaint you, that he (Mr. *John Ingram*, of this Place, Watch-maker and White-Smith, whose Father, originally a Black-Smith at *Cowbitt*, and Inventor of the Machine for cutting Watch-Wheels, was also a most accurate Artificer) having made, and, at my Instance some time since, fixed up in our *Museum*, a Metal Thermometer, which we, on Experience and Observation, found to answer and go truly, I proposed to the Company, at our last Meeting in *December*, that our *Society* should purchase it him, I send you, Sir, his Description thereof, as enter'd from his Mouth in our Minutes, which I trust may be agreeable to you, and the worthy Members of that Illustrious Body, for which we here have the highest Honour: And though Mr.

Beridge



page 129

Beridge (some time a Watch-maker at *Boston*) under the same Inventor's Direction, made and carried up to Town a Machine somewhat of this sort, which several of your Members may have seen, yet I trust this Account may not be unacceptable. I am

Your most affectionate humble Servant,

M. Johnson.

*A Description of the Metal Thermometer in the
Museum of the Gentlemens Society at Spalding
in Lincolnshire.*

IT is composed of an upright Staff or Bar (*a*) of the best Iron, four Feet long, and an Inch and a Quarter broad, having a polish'd brass Bar of the same Length and Width screw'd to it before it, with four Steel Screws, and being also capp'd (*b*) with Steel, and thereon a Lever (*c*) moving upon a Stud of Steel, which communicates with another less Lever (*d*) (also upon a Stud) having a Chain (*e*) at the End of it, which laps round an *Axis* (*f*), whereto the Index is fixed, which shews the Degrees marked on a semicircular Arch (*g*): Under the Steel Screw-Heads there are small Slits in the Brass Bar (except the lowermost which is fixed) which admit of its expanding, whereby it protrudes and operates on the first-mention'd Lever, which being raised moves the less Lever, and thereby draws the Chain which turns the *Axis* fixed to the Index, which shews the Degree

Degree of Warmth of the Weather marked on the semicircular Arch. At (b) is a Screw thro' two Studs, to draw the great Lever backwards and forwards, as Occasion may be; (i) is a Counter-balance to the small Lever to draw the Hand back when the Brass Bar shrinks. See the Figure prefixed in the TAB.

In the Beginning of the Year 1735. I invented, and caused to be constructed, a Thermometer on the same Principles as this: I found that a Rod of Brass 3 Feet long was sensibly affected by the Changes of Heat of the Weather, having one exposed in my Garden during the hard Frost of the Winter 1739 and 40. And my Instrument was very sensible with either a Brass Rod or an Iron Rod, when the Bottom of it was placed in a Sand-Heat for chemical Uses; but I shall refer the Reader to the Appendix to the preceding N^o. p. 672. & seq. wherein I have given, a full Description of my Invention, and the Reasons why I did not publish it before; tho' I have shewn the Instrument to Scores of People ever since May 1735. and sent a Description and Draught of it to M. Buffon, Superintendant of the Royal Physick Garden at Paris in the Year 1744. in order for his laying it before the Royal Academy of Sciences at Paris, from which I had some time before received a Diploma upon having the Honour of being appointed, one of their Corresponding Members.

C. Mortimer.

ERRATA.

Page 1. l. 9. for Febr. 14. read Jan. 7. and 14. P. 27. in the Column expressing the Nutation,

1729. Sept. 8. for — 6.9 read — 6.4

1730. Sept. 8. for — 3.4 read — 3.9

PHILOSOPHICAL TRANSACTIONS.

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*The Original read
at a Meeting of the
Royal Society, Feb.
4. 1747-8.*

THE Particulars of this extraordinary Case are set forth at large in * *Dr. Mounsey's* Paper, the Substance whereof is as follows:

A Soldier's Wife, of *Abo* in *Finland*, of a middling Stature, and who had been the Mother of two Children, being pregnant a third time in the Year 1730. was afflicted with violent Pains and Twistings in the Bowels, swooning Fits, Vomitings, and great Disorders in her Back and lower Belly. These Symptoms, and Complaints of several other kinds, continued to make her uneasy, till she found her Burden

* It is likewise printed in the *Swedish* Language, in the Acts of the *Royal Academy* at *Stockholm*.

Burden increase, which fell from Side to Side as she changed the Posture of her Body, and could be pressed by her Hand from one Place to another, but rather chose to remain on the right Side.

After Quickening her Health became better, she grew bigger belly'd than ordinary, and was supposed to carry Twins. About the time her Delivery was expected, she was taken ill with violent Pains by Fits across her lower Belly; but had none in her Back, nor any Forcings downwards; and next Day these Pains went off, which made her suppose she had misreckon'd. But after this her Breasts swell'd, and gave Milk in Plenty; and her Menfes came on, attended with more violent Pains than she had had before; and such large Discharges of Blood from time to time, that she could neither speak nor move; and even after the Floodings were stopp'd, her Blood and Strength seem'd quite exhausted: She often fainted away, and was sometimes thought to be dead.

She continued very sickly for ten Years afterwards; during which Time her Burden was moveable, and fell from Side to Side. But in the Month of *September* 1741. she felt a Pain beneath the Navel, with a Swelling and Redness, which in about three Weeks appear'd like a small Boil. This she pierced with an Awl, and a yellow-colour'd Water ran from it without any Smell, and continued so to do for near three Weeks more, when it discharged a purulent stinking Matter.

In the Month of *June* two small Bones came out, which were given to the Surgeon that visited her; who only applied a Piece of Plaister, persuading her that a Cure was impossible. Other small Bones
work'd

work'd themselves out afterwards, till *October 1742.* when the Head-Quarters of the *Russian Army* being at *Abo*, this unhappy Woman applied to Dr. *Mounsey*, who, after a careful Examination, undertook to deliver her. And accordingly, desiring the Assistance of Mr. *Geitle*, Surgeon to the Regiment of *Abo*, a grooved Probe was thrust into the *Fistula*, and an Incision made with a Bistoury, upwards and obliquely, from the *Linea alba* into the Cavity of the *Abdomen*; but she being unruly, and the Operation not going on to the Doctor's Liking, he proceeded no farther till the next Day; only some loose Bones were extracted, and the Wound dressed with Tents and Compresses, to hold in the *Omentum*, and keep the Wound open.

At the next Operation the Incision was carried downwards, and then another Incision continued from the first was conducted upwards, and slanting at a small Distance from the first; taking care to keep as near as possible the Direction where the Adhesion of this Body to the *Peritonæum* appeared to be, and avoiding to make the external Wound larger than absolutely needful, lest the *Omentum* and Guts should fall out; and particularly lest the Suppuration should exceed the Strength of Nature, which was here already nearly exhausted.

A large Opening was now made, but the *Fœtus* being closely enveloped by its containing Sack, the Doctor durst not venture to draw it out by Force, for fear some of the naked Bones might lacerate the internal Parts: Wherefore, dilating this Sack with the Points of a Pair of Probe-Scissars, directed by the Fingers of the left Hand, he pierced

and cut in Pieces the Skull, which afterwards he extracted piece-meal.

The Matter that first issued out had a very nauseous Smell, and consisted of Membranes, Fat, and corrupted Flesh. On opening the *Cranium*, the *Cerebrum* appeared of its natural Colour. The Operation having been long, and the Woman fainting away, the Wound was dressed, without attempting to extract more at that time. In the Evening she was taken with Vomitings; but by proper internal Medicines, and Flannel Stoups wrung out of hot Wine, applied over the whole Belly, and often renew'd, she found Ease, and grew better. — The Loss of Blood, during the whole Operation, was inconsiderable,

Next Day the Bones of the Trunk, and most of the other large ones, with their Ligaments and rotten Flesh, were taken out. The Matter discharged for several Days was of a dark-brown Colour, occasion'd by Blood issuing from the dilated Pores of the internal Surface of the Sack, which render'd the Matter at first of a deep-red Colour; but that changed daily and gradually, till at last it became white. The Doctor imagines this Discharge to resemble the *Lobchia* after Child-birth; for after the *Fœtus* was extracted, the Woman's Breasts swell'd, and gave Milk in Plenty for two Months, in Quantity, Colour, and Consistence, as if after a Delivery at the proper Time.

The Doctor examined this Sack very diligently with his Fingers, whilst thin, and not contracted, but formed into Wrinkles, through which he felt the *Rectum*, the *Vesica urinaria*, and, as he thought, the

the *Fundus Uteri*. Many small Bones lay in the Folds; but, as she complained not of Pain, they were left till the Suppuration began, excepting those the Sack in contracting itself threw out.

Besides Fomentations, Balsamics, proper Bandages, &c. vulnerary detergent Injections were found very useful, thrown in in large Quantities, both to wash out the putrid Flesh, and bring away the Scales of Bones which were still concealed in the Folds of this Sack.

The Sack contracted itself daily, grew smooth and white within, and narrower as it approached the *Uterus*, which gave Reason to believe it one of the *Fallopian Tubes*.

The Wound was cured in about six Weeks, and the Woman deliver'd from a long State of Misery grew fat and lusty, and now enjoys good Health.

Comparing these Circumstances together, it seems reasonable to believe this Fruit never was in the Cavity of the Womb, but that the impregnated *Ovum* was stop't in its Passage through one of the *Fallopian Tubes*, where it grew, and was detained so many Years; and that the Inflammation, which happened below the Navel, was not owing to the Rottensness of the *Fœtus*, or to its bare Bones seeking a Discharge, but rather to some accidental Friction of the containing Sack against the *Peritonæum*, thereby producing Adhesion, Obstruction, Inflammation, &c. The *Fœtus*, before this Accident, must have remained all these Years intire, and without perfect Corruption: For it took no less Time, after its Communication with the common Air, before it
shewed

shewed Marks of Putrefaction, than a fresh Subject, kept in the same Degree of Heat, would have done.

The Doctor's Observations of the Bones are : That they have a full Proportion to those of a Child at nine Months, and that the Fibres are more compact, and their Articulations stronger. The Sockets for the Teeth are six on each Side of the Jaws; the *Dentes Incisores* of the upper Jaw are high and large; the *Molars* have almost all begun to ossify in their *Alveoli*; at least the Crown, which is the cortical Part, is form'd, and they are fill'd internally with a cretaceous Substance. In new-born Children those Parts are seldom found so far advanc'd, which gives Reason to believe this Child did not die within the ordinary Time of Pregnancy, and that the different Accidents, before-mention'd to have happen'd, were chiefly owing to the preternatural Situation of the *Fœtus*.

Some Places in the Skull appear to have been carious, and corroded by some sharp Humours; and Nature, supplying its ossifying Juices, has repaired these Places, and render'd them more solid and whiter than the rest, but very uneven and scabrous, from the different Times and Directions of the bony Sproutings. There are likewise *Exostoses* on the Ends of the Thigh-Bone, and some other Bones.

'Tis very difficult to determine about what Time the Growth of these Productions began or ended. Supposing it from the Time of the Disorders that happened in the first Months of Pregnancy; would not such a Disease have caused Death to the *Fœtus*, before it had come thus to a full Growth? If it was the Consequence of the violent Accidents which
happen'd

happen'd about the Time of the natural Birth, the Child then must have continued alive some considerable Time afterwards, during which these bony Excrescences were formed; there being a perfect Ossification, as performed by the Laws of Circulation, and not by any vegetative or petrifying Power, as in inanimate Bodies.

Two or three of the lateral Processes of the Spine were what first pass'd thro' the little Ulcer; the rest of the Bones (except a few that were lost in cleaning) were presented by the Doctor to the Museum of the *Royal Society*. They retain a very strong and singular Smell, though they were immediately cleansed from the rotten Flesh, and well washed.

The Woman came by Sea to *Stockholm* above a Year after this Cure, and was presented to the *Academy* in good Health; and the Doctor believes she is still alive and well.

II. *The Motion of Projectiles near the Earth's Surface consider'd, independent of the Properties of the Conic Sections; in a Letter to Martin Folkes Esquire, Pr. R. S. by Mr. Tho. Simpson F. R. S.*

Read Feb. 4.
1747.

AFTER so much as has been already said upon the Motion of Projectiles in ~~vacuo~~, it may seem needless to attempt any thing further on that Head; nevertheless, as a thorough Knowledge in the Art of Gunnery is become more than

than ever necessary, and as Gentlemen employ'd in the Practice of that Art are (I am sensible) too often deterr'd from applying themselves to the Theory, by the Difficulties they imagine they shall meet with in the Conic Sections, you will, I hope, pardon the Liberty I have taken, in troubling you with my Thoughts on a Subject, in which little or nothing new is to be expected besides the Method.

When I first drew up this Paper (which was about two Years ago) I did intend, had Health permitted me to make the proper Experiments, to have also attempted something with respect to the Resistance of the Atmosphere, whereof the Effects are indeed too considerable to be intirely disregarded: But if the Amplitude of the Projection, answering to one given Elevation, be first determined by Experiment (which our Method supposes) the Amplitudes in all other Cases, where the Elevations and Velocities do not very much differ from the first, may be determined, by the Proportions here laid down, to a sufficient Degree of Exactness: Because, in all such Cases, the Effects of the Resistance will be nearly as the Amplitudes themselves; and were they accurately so, the Proportions of the Amplitudes, at different Elevations, would be exactly the same as *in vacuo*; which Proportions I now proceed to determine.

PROBLEM I.

Let two Balls be projected with the same Celerity, at different, but given Elevations, 'tis proposed to determine the Ratio of the Times of their Flight,



7

8



Flight, of their greatest Altitudes, and of their horizontal Amplitudes.

Let Pq (Fig. 1.) represent the Plane of the Horizon, PEQ and peq the Paths of the Projectiles, described in the Flight; moreover let QPT and qpt be the given Angles of Elevation, and let PQ and pq be bisected in H and h ; drawing HE , QT , he and qt , all perpendicular to Pq ; and making the Sine of $QPT = S$, its Co-sine $= C$, the Sine $qpt = s$, its Co-sine $= c$, and Radius $= r$.

Therefore, since the Distances descended by heavy Bodies (whether from a Point at Rest, or from the right Lines in which they *would* move, if not acted upon by Gravity) are known to be as the Squares of the Times, QT will be to qt ; as the Square of the Time of describing PEQ (or of that wherein the Ball would move uniformly over the Space PT with its first Velocity at P) is to the Square of the Time of describing peq (or of that wherein the other Ball would move uniformly thro' the Length pt). But the Celerities at P and p being equal, by Hypothesis, the Times in which the said Lines PT and pt would be uniformly described, are manifestly, as the Lines themselves: Whence the Squares of those Lines must, also, be as the Squares of the Times, and, consequently, as the Distances descended: that is, $PT^2 : pt^2 :: TQ : tq$.

Now, by Plane Trigonometry $TQ = \frac{S \times PT}{r}$ and $tq = \frac{s \times pt}{r}$; therefore $PT^2 : pt^2 (:: \frac{S \times PT}{r} : \frac{s \times pt}{r}) :: S \times PT : s \times pt$; whence, by dividing the Antecedents by T by

by PT , and the Consequents by pt , we have $PT : pt :: S : s$; from which it appears, that the Times of Flight are directly as the Sines of Elevation.

Again, the Times of describing EQ and eq (which are the Halves of the Wholes) being also to one another as $S : s$, and the Distances EH , eh descended in them, as the Squares of the Times, it likewise follows, that $S^2 : s^2 :: EH : eh$; or that the greatest Altitudes are as the Squares of the Sines of Elevation.

Moreover, because (by Trigonometry) $PT = \frac{r \times PQ}{C}$ and $pt = \frac{r \times pq}{c}$, and it has been already proved,

that, $S : s :: PT : pt$, it follows, that $S : s :: \frac{r \times PQ}{C} :$

$\frac{r \times pq}{c}$; whence, by multiplying the Antecedents by $\frac{2C}{r}$ and the Consequents by $\frac{2c}{r}$, it will be $\frac{2SC}{r} : \frac{2cs}{r}$

$(:: 2PQ : 2pq) :: PQ : pq$. But $\frac{2SC}{r}$ is known to be

the Sine of double the Angle whose Sine is S , and Co-sine C , &c. Therefore the horizontal Amplitudes are to one another, as the Sines of the double Elevations.

Corol. 1.

Hence it follows, that the greatest Amplitude possible will be, when the Elevation is half a Right Angle, or 45 Degrees (because the Sine of 90° is the greatest of all others).

Corol.

Corol. 2.

Therefore, if the greatest Amplitude be given (from Experiment) the Amplitude answering to any proposed Elevation, above, or below, 45 Degrees, may from hence be found: For it will be as the Radius, to the Sine of double the given Elevation, so is the greatest, to the required, Amplitude.

Corol. 3.

Hence, also, the Altitude of the Projection may be known; for QT , when the Angle QPT is half a Right Angle, will be $= PQ$; and therefore HE ($\frac{1}{2}TQ$) $= \frac{1}{2}PQ$; also, in this Case, $S^2 = \frac{1}{2}r^2$; whence our Proportion $S^2 : s^2 :: HE : be$ will here become $\frac{1}{2}r^2 : s^2 :: \frac{1}{2}PQ : be$; from whence it appears, that, as the Square of the Radius is to the Square of the Sine of any given Elevation, so is half the greatest horizontal Amplitude, to the Altitude of the Projection. Hence it also follows, that the Height to which the Ball would ascend, if projected directly upwards, is just half the greatest Amplitude.

Corol. 4.

Therefore, since it is well known, that a Body *in vacuo* ascends and descends with the same Velocity; and that the Distances descended are as the Squares of the Velocities; it follows, that the Amplitudes, at the same Elevation, with different Velocities, will also be to one another as the Squares of the Velocities; because they are as the greatest Amplitudes, with the same Velocities (by Corol. 2.)

and these are as the Distances perpendicularly descended (*by the precedent*). Whence, *universally*, if both the Elevations and the Velocities differ, the Amplitudes will be to each other in a *Ratio* compounded of the *Ratio's* of the Sines of double the Angles of Elevation, and of the duplicate *Ratio's* of the Velocities, or impelling Forces.

Problem II.

The Angle of Elevation, and the greatest horizontal Amplitude, being given, to find at what Distance the Piece ought to be planted, to hit an Object, whose Distance, above or below the Plane of the Horizon, is also given.

Let *AB* (Fig. 2 and 3.) be the Plane of the Horizon, *BC* the perpendicular Height or Depression of the Object, and *AB* the required Distance: Also let *BC* be produced to meet the Line of Direction *AD* in *D*, and let *P* be the Place where the Path of the Projectile would meet the Horizon; moreover, let *PQ* be perpendicular to *AP*, and *CN* parallel to *AD*. Then, by the preceding Problem, it will be as Radius: the Sine of $2BAD ::$ the given (or greatest) Amplitude: *AP*; which therefore, is known.

Moreover, the Areas of similar Triangles being as the Squares of their homologous Sides, we have $AP \times PQ : AB \times BD :: AQ^2 : AD^2$. But $AQ^2 : AD^2 :: AB \times BD :: QP : DC$ (from Principles already explained) therefore, by Equality, $AP \times PQ : AB \times BD :: QP : DC$; and consequently $AP : AB :: BD : CD$; but (because of the parallel Lines *CN* and *AD*) $BD : CD :: AB : AN$; whence,
again

again by Equality, $AP : AB :: AB : AN$; therefore, by Division, $AP : BP :: AB : BN$; and, consequently $AP \times BN = BP \times AB$.

Let AP be now bisected in O ; then $BP \times AB$ being $= AO^2 - OB^2$ (in the first Case) and $= OB^2 - AO^2$ (in the second Case), we shall therefore have $OB^2 = AO^2 \mp AP \times BN = AO \times \overline{AO \mp 2BN}$: whence the Distance AB is likewise known. *Q.E.I.*

Corollary.

Hence, if the Elevation, and the greatest Amplitude, together with the Distance AB of the Object be given, the Height or Depression of the Ball in the Perpendicular BCD will be known: For it is proved, that $AP : BP :: BA : BN$; whence BN is known: But, as the Radius to the Tangent of BNC (BAD): so is BN to BC .

Problem III.

The greatest horizontal Amplitudes of the Piece, together with the Distance and Height (or Depression) of the Object being given, to find the Direction or Angle of Elevation.

Let BC (Fig. 4. and 5.) be the perpendicular Height or Depression of the Object, AB its given horizontal Distance, and AH the required Direction; Also let PQ (Fig. 6.) be the greatest Amplitude (answering to 45° of Elevation); draw AC , in which produced (if need be) take $AG = PQ$; make MGO perpendicular to AG , meeting AB produced (if need be) in O ; and from the Centre O , with the Interval OA ,
let

let a Circle be described, intersecting AG , produced in E , and the Line of Direction AD in H ; join E, H , and let HI , AN and QR , be perpendicular to AE , AO , and PQ respectively, and let BC , produced, meet AH in D .

It will appear, from what has been said above, that $AD^2 : PR^2 :: DC : RQ$; therefore PR^2 being $= 2PQ^2 = 2AG^2 = \frac{1}{2}AE^2$, and $RQ = PQ = \frac{1}{2}AE$ (by Construction), we have $AD^2 : \frac{1}{2}AE :: DC : \frac{1}{2}AE$, and therefore $AD^2 = AE \times DC$.

Now, the Triangles ADC , AEH , being equiangular (because $ADC = DAN = AEH$, and DAC common to both) we likewise have $AD : DC :: AE : EH$, and consequently $AE \times DC = AD \times EH = AD^2$ (per above); whence $EH = AD$. Therefore, as the Triangles ADB and EHI are equiangular, they are equal in all respects; and so $HI = AB$: Whence follows this easy

Construction.

Having described the Circle AEF , as above directed, and drawn MG perpendicular to AE , take Gn equal to AB , and thro' n , parallel to AE , draw Hh , cutting the Circle in H and h ; join A, H , and A, h ; then either of the Directions AH or Ah , will answer the Conditions of the Problem. From this Construction we have the following Calculation; viz.

As AB is to BC , so is AG to OG ; which added to, or subtracted from, Gn (AB) gives On : Then, it will be, as $AG : On ::$ the Co-sine of OAG : Co-sine of HOn ($= HAh$) the Difference of the two required

required Elevations; whence the Elevations themselves are known. *Q. E. I.*

Corol. 1.

Hence, if the Elevation of the Piece, with the Distance and the Height (or Depression) of the Object be given, the greatest horizontal Amplitude may be found: For it will be $AB:BC::\text{Radius:Tang. of } BAC$; whence CAD is also known.

Then, $S. CAD : S. ACD (AHE) :: AD (HE) : AE$.

And, $S. ADC : \text{Radius} :: AB : AD$.

Therefore, by compounding these Proportions, we have $S. CAD \times S. ADC : \text{Radius} \times S. ACD :: AB : AE$; which is equal to twice the required Amplitude, by Construction.

Corol. 2.

Moreover, if the Elevation, and the greatest horizontal Amplitude be given, the Amplitude of the Projection on any ascending or descending Plane AE , whose Inclination FAE is also given, may from hence be derived. For, $S. AHE (ACD) : S. EAH (CAD) :: AE (2PQ) : EH (AD)$ and $S. ACD : S. ADC :: AD : AC$; whence, by compounding the two Proportions, $Sq. S. ACD : S. CAD \times S. ADC :: 2PQ : AC$; from which AC is known.

Corol. 3.

Since it appears, that the Triangles ADB and EHI are equal and alike in all respects, and, therefore, the horizontal Distance AB , *universally*, equal to the Perpendicular HI , it is manifest, that, when

HI

HI is the greatest possible, *AB* will also be the greatest possible; in which Circumstance *AC* (if the Angle *FAE* be given) will likewise be the greatest possible: And this, it is evident, must be, when *HI* coincides with *MG*, or when the Angles *HEA* and *HAE* are equal (as in *Fig. 7* and *8*); at which time the Point *D* coincides with *H*; because *AD* and *EH* are always equal to each other. Therefore, since, in this Case, *HAE* (*HEA*) is $= NAH$, it follows, that the Amplitude, on any inclined Plane *AE*, will be the greatest possible, when the Line of Direction *AH* bisects the Angle made by the Plane and Zenith.

Corol. 4.

Hence the greatest Amplitude on any inclined Plane may also be known; for the right-angled Triangles *AOG* and *HOB*, having $AO = HO$ and the Angle *O* common, are equal in all respects; and therefore, as Tang. of *AHG* (*BAH* the Angle of Elevation): Tang. of *CHG* (*CAB* the Plane's Inclination) $:: AG : GC$; whence $AC = AG \mp GC$ is also known.

Corol. 5.

Hence, also, if the greatest Amplitude on an inclin'd Plane be given, the greatest horizontal Amplitude may be determined: For, Radius $S. BAC :: AC : BC = CG =$ the Difference of the given, and the required, Amplitudes.

Corol. 6.

But if, instead of the Plane's Inclination, the perpendicular Height, or Depression, of the Object be given; then, *AC* ($AG \mp BC$) being to *BC*, as Radius

to the Sine of BAC , and Radius : Cotang. BAC :: $BC : AB$; the greatest Distance AB , at which the Ball can possibly hit the Object, will from hence be given: which Distance (because $AC = AG \mp BC$, and $AB^2 = \overline{AC \mp CB} \times \overline{AC \mp BC}$) will also be expressed by $\sqrt{AG \times AG \mp 2BC}$. Hence the greatest horizontal Amplitude of a Ball; projected from a given Height above the Plane of the Horizon is known: For ST (*Fig 8.*) may here be supposed to represent the Plane of the Horizon, and SA the given Height; and then SC , being equal to AB , is given from above = $\sqrt{AG \times AG \mp 2BC}$.

Corol. 7.

But, if the horizontal Distance AB be given, and it be required to find the greatest Height the Ball can possibly reach in the Perpendicular BCD ; we shall have $HG (AB) : AG ::$ Radius : Tang. of the Elevation (BAH or AHG); and Radius : Tang. BAC ($2BAH \approx 90^\circ$) :: $AB : BC$; which therefore is known. But (because $AC \pm BC = AG$, and $\overline{AC \mp CB} \times \overline{AC \mp CB} = AB^2$) the same will also be truly exhibited by $\frac{AG^2 \approx AB^2}{2AG}$.

Corol. 8.

Lastly, let the Height, or Depression, of the Object be given, together with its Distance AB , to determine the Direction; and the least *Impetus* possible, to hit the Object: Then $AB : BC ::$ Radius : Tang. BAC ; whence the Elevation BAH is known: And as Radius : Tang. $AHG (BAH) :: MG (AB) : AG$; whence the *Impetus* is also known.

III. *The Case of Henry Axford, who recover'd the Use of his Tongue, after having been four Years dumb, by means of a frightful Dream; communicated by the Rev. Mr. Archdeacon Squire, F.R.S.*

Read Feb. 4. 1747-8. **H**ENRY Axford, Son of Henry Axford, of the *Devizes* in *Wiltshire*, an Attorney, when a Child was subject to Convulsion-Fits, which followed him pretty frequently till he was about 25 Years of Age. After this, his Health became extremely good. At about 28 Years old, going with some Ladies to see *Longleat* in *Wiltshire*, the Seat of Lord Viscount *Weymouth*, he perceived a Hoarseness coming upon him, which was soon after attended with all the Symptoms of a common Cold, till, in about six Days after his first Seizure, he became quite speechless, not only losing the articulate Use of his Tongue, but being scarcely able to make the least Noise with it. His Cold quickly went off in the usual manner, and he grew perfectly well, as well in Health as ever he had been in his Life; but he still continued absolutely speechless. He had Advice from all the neighbouring Physicians, but to no purpose; for nothing they did for him could restore him to the former Use of his Tongue.

He continued in this dumb Way about four Years; till one Day in the Month of *July*, in the Year 1741, being at *Stoke* in the above-mention'd County, he got very much in Liquor, so much, that, upon
his

his Return home at Night to the *Devizes*, he fell from his Horse 3 or 4 times, and was at last taken up by a Neighbour, and put to Bed in a House upon the Road. He soon fell asleep; when, as he tells the Story himself, dreaming that he was fallen into a Furnace of boiling Wort, it put him into so great an Agony of Fright, that, struggling with all his Might to call out for Help, he actually did call out aloud, and recovered the Use of his Tongue from that Moment as effectually as ever he had it in his Life, without the least Hoarseness remaining, or Alteration in the old Sound of his Voice, as near as can be discerned. He was not used to drink hard; he is still alive, continues in good Health, and has the Use of his Tongue as perfectly as ever he had in the former Part of his Life.

IV. *Extract of a Letter from Mr. William Arderon, F. R. S. to Mr. Henry Baker, F. R. S. concerning the Hearing of Fish.*

S I R,

Read Feb. 11,
1747-8.

AS it was at your Desire that I set myself to make Experiments and Observations on the Nature and Properties of Fish, and to discover, if possible, whether they are sensible of Noises, and of the Motions of Bodies, by Hearing, in the Manner of Land Animals; or whether, being destitute of that Sense, the Want of it is supplied by the Quickness of their Sight and Feeling; I am going to lay before you the Method I made

use of to come at some kind of Certainty in this doubtful Affair; and shall think my Time has been well employed, if it can afford you any Satisfaction.

Tho' Fishes are not provided with Organs for Hearing, similar to those serving to that Purpose in other Animals, it would be too presumptuous to declare, without Experiment, that they are unable to hear, by Organs differently placed, whose Situation and Structure, for want of due Examination, we are unacquainted with.

In order therefore to be able to judge from real Facts, without being in the least prejudiced by what has been written for or against their Capacity of Hearing, I have, for almost three Years past, been continually trying Experiments on several Kinds of Fishes; *viz.* Perches, Ruffs, Bansticles, Millers Thumbs, Minnows, &c. which I have kept in Glass Jars for that Purpose; and at the Hours of feeding them, as well as at other Times, have, by different Noises, such as Whistling, Halloing, the Sounds of several musical Instruments, and every other means I could contrive, endeavoured to discover their Sense of Hearing, if they were indeed endowed with that Sense; but could never perceive they were affected by any of these Noises.

But whether Fishes do or do not hear, it is certain their Senses of *Feeling* and *Seeing* are exquisitely quick; and I believe, by the extreme Sensibility of these two, one may explain most of the Accounts that have been brought by Writers as Proofs of their Hearing; such as their coming, when called by their Names, as *Plutarch* relates of *Marcus Crassus's* Lamprey; their flocking in Throngs
when

when called to be fed; as Mr. *Bradley* tells us he saw the Carps do in the Pond of one Mr. *Eden* at *Rotterdam*; and their flying away from the Halloing and Noises made by Sailors, as *Wolfgangus* reports the Dolphins do, when the Sailors have a Mind to fright them. — But may we not as reasonably imagine these Dolphins fly from the Sailors, their Ships and Boats, on account of the violent Action wherewith such Halloings usually are performed, as merely on account of the Noise they make? And in the other Cases, is it not as probable, that the Fish in Ponds, either by their Sight or Feeling, discover'd the Approach of their Benefactors, whose coming they were accustomed to expect, as that they were sensible of their Voices calling them?

I have often struck with my Thumb-Nail against the Edge of a glass Jar, in which I kept two Ruffs, a Stroke not harder than the Beat of a Pulse, which would cause them in a Moment to dart from the Bottom of the Jar to the Top; tho' I am sure they did not see me. But if I made the same Motion without hitting the Glass, or if I made an hundred times louder Noises than the striking of my Nail against the Glass, at a very small Distance from it, I could not perceive they were in the least affected thereby; which, if duly consider'd, may I think amount to a Proof of the Deafness or Want of Hearing in this kind of Fish at least; and that their delicate Sense of Feeling supplies them with the Knowledge of the Motions of Bodies, when their other Senses fail. Indeed I have often been convinced by Experiment, that their Feeling is exceedingly acute, perhaps more so than in other Animals; whence I have been led
to

to imagine, that their Fins may possibly be the Organs more immediately sensible of the slightest Motions in the Medium wherein they dwell. The Curious, who have observed the Fins of Fishes with the Microscope, find them to be composed of infinitely fine Vessels, Arteries, Veins, Muscles, and membranous *Fibrillæ*, whose Structure seems more delicate than is necessary for Parts that serve only as Oars to waft the Fish along. This however I desire you to regard as a mere Conjecture, for which the necessary Proofs are wanting.

At other times, if, by striking on the Top of the Jar with a small Key, the Stroke or Tremor has been a little more violent, the Fish would shut down their back Fins in a Moment, and remain motionless at the Bottom of the Glass. The sudden Appearance of my Hand at the Top of the Jar would likewise produce the same Effect; but Noises made near them seemed to give them no Disturbance.

These Experiments I have often repeated before several of my Acquaintance, as well as by myself in private, and seldom found them to vary much. But Fish newly taken out of Ponds, or Rivers, must not be expected to perform all I here relate: For they, like Birds just taken in the Fields, and put in Cages, are thrown into Confusion at the Approach of any thing, and endeavour continually to regain their Freedom.

If the Eyes of Fishes be carefully examined, when swimming in a glass Vessel, the *Cornea* or black *Uvea* of their Eyes may be seen, sometimes advancing forwards, and at other times retiring back, just as their Sight is directed to near or distant Objects,

jects, through a grosser or finer Medium; the Form of their Eyes altering, as the Occasion requires, to make them distinguish Objects; and their Eyes have so great a Liberty in the Orbits, that they are able to turn them any Way, upwards, downwards, to one Side or the other, nearly a Quarter of a Circle, which makes them full amends for the want of Motion in their Necks, and enables them to change or direct their *Optical Axis* to any designed Place in a Moment.

Those who have been accustomed to Fly-Fishing can bear Witness, that the Sight of Fishes is quick and distinct almost beyond Belief: For it is not uncommon to behold a Fish dart itself 20 or 30 Yards in an Instant at a Fly thrown out at the End of a long Line, and catch it even before it can well touch the Water. Few other Creatures are perhaps capable to distinguish Objects so small at so great a Distance, at least not so perfectly as these do; for, let the artificial Fly differ in Colour, Shape, or Bigness but very little from the natural one it should represent, and not a Fish will meddle with it.

These Instances of the exquisite *Feeling* and *Seeing* of Fishes, together with their Want of Organs that can be certainly known to serve them for *Hearing*, as well as of sufficient Facts to prove that they do hear, may, I think, amount to the highest Probability, that they are really destitute of that Sense *, and stand in no need thereof, notwithstanding

* 'Tis not hereby denied, that Fishes of the cetaceous Kind may probably hear, as well as some other Kinds produced in the Sea, that have Parts in common with Land Animals. These Observations are confined to the common Fish of our Rivers.

standing the contrary Opinions of some Authors: And their living in an Element, where Land Animals are capable of remaining but a very short time, may render an absolute Certainty in this Case unattainable.

But in order to discover what Land Animals can do, or what Fish, had they Organs of Hearing similar to those of Land Animals, would be capable of doing, I endeavoured last Summer to find out by Experiment,

First, Whether or no Sound made in the open Air can be heard by a Land Animal immersed under Water.

Secondly, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal in the open Air. And,

Thirdly, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal that is likewise under Water.

To satisfy my first Inquiry, whether Sound made in the open Air can be heard by a Land Animal under Water; I caused three People, stript quite naked, to dive down at the same time, and to remain about two Feet below the Surface of the Water; in which Situation I spoke to them as loud as I was able. At their coming above Water, they repeated my very Words, but said I spoke very low.

I caused the same Persons afterwards to dive down about 12 Feet under Water, and a Gun was discharged over them, which they all said they heard, but that the Noise was scarce perceivable.

As to my second Inquiry, Whether, and in what Manner, Sound made under Water can be heard in

in the open Air: I caused a young Man to dive some Feet down, and then to endeavour to halloo, which he did; and I could hear him, though very faintly. But imagining the Sound might come up with the Water he discharged at his Mouth whilst he halloo'd, I contriv'd a kind of Hand-Granado, which I threw into a Place in the River about nine Feet deep. The Fuzee burnt under Water near 10 Seconds, and then the Granado went off, giving a prodigious hollow Sound, and shaking the adjacent Ground to such a Degree, that the Whole of a large Building, some Yards distant from the Explosion, was put into a Tremor, far beyond what could be expected from so small a Quantity of Powder.

I satisfied my third Inquiry, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal that is likewise under Water, by procuring a young Man to dive down with a Bell in his Hand; and he assured me, that he heard its tinkling very distinctly, at all Depths under Water, with little or no Difference from what he did when rung in the open Air: He likewise affirmed, that he plainly heard the Noise and Rushing of the Water, which came violently through a Flood-Gate, about 20 Feet distant from the Place he then was in.

If these Experiments and Observations may be thought deserving Notice, I shall think my Time not thrown away; but at all Events be assured, that I am,

Dear Sir,

Norwich, Nov. 27.

1747.

Your most obliged humble Servant,

W. Arderon.

V. *The Substance of some Experiments of planting Seeds in Moss, lately made by Mr. Charles Bonnet, of Geneva, F. R. S.*

Read Feb. 18. 1747-8. **M**R. *Bonnet* was inclined to try whether Plants were capable of Vegetation, when they were only set in Moss, instead of being planted in the Earth.

With this Design, he filled with Moss several Garden Pots, and he compressed the Moss more or less, as he judged, the several Plants he intended to place in them, might respectively require a closer or a looser Soil.

He then sowed in Moss, Wheat, Barley, Oats, and Pease. And he found, first, that all the Grains sowed in that manner came to Maturity later than those of the same Sorts which were sowed at the same time in Mould.

2dly. That the Stems from the several Grains sowed in the Moss were generally taller than those which sprung from the Ground.

3dly. There came from the Grains sowed in the Moss a greater Number of Blades than from the Grains sowed in the Earth.

4thly. The Grains sowed in Moss produced more plentifully than the others.

5thly. Those Grains that were gather'd, from the Produce of those which vegetated in the Moss, having been again sowed some in Moss, and some in Earth, succeeded well in both.

Mr. *Bonnet* has also planted in Moss, Pinks, Gilly-flowers, Daiesies, Tuberoses, Tulips, Hyacinths, Jonquils, and Narcissus's; and all these Plants succeeded as well

well as others of the same Sorts, which he at the same time planted in Mould.

He also placed in Moss Cuttings and Layers of Vines, and these Cuttings and Layers became Vines; and these Vines in a short time grew larger than others, that came from Cuttings and Layers planted at the same time in the Ground.

VI. *A Continuation of an Account of an Essay towards a Natural History of Carolina, and the Bahama Islands; by Mark Catesby, F. R. S. with some Extracts out of the Appendix, * by C. Mortimer, Secret. R. S.*

Read Feb. 18. 1747-8. *Urogallus minor, fuscus; cervice plumis alas imitantibus donatâ.*

This Bird was about a third Part bigger than a common Partridge, all over of a reddish Brown, marked transversely with black and white waved Lines intermixed; but what is singular and extraordinary in this Bird, and distinguishes it from all others yet known, are two Tufts of Feathers resembling little Wings, 3 Inches long, placed on the hind Part of the Head, opposite to one another. These little Wings (if so they may be called) were fixed, as our Author says, to the Neck, in like manner as the real Wings are to the Body; whereby it had the like Power of contracting and dilating them; and they may

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possibly

possibly assist the Bird, in running, or flying, or both, especially as the Wings are short in proportion to its heavy Body. They are Natives of the Northern Parts of *America*. Lord *Wilmington* had one of these Birds at *Chiswick*.

Meadia. So called in Honour of Dr. *Mead*.

The Leaves of this Plant resemble those of a Lettuce, from which rises a single Stalk, about a Foot high; on the Summit of which are fixed many rectilinear Footstalks, on every one of which hang pendant a single purple Flower, which is monopetalous, somewhat resembling the Autumn *Cyclamen*. This is a very ornamental Plant, and flowers yearly at Mr. *Collinson's* Garden at *Peckham*.

2. *Scolopendra*. This is a very venomous Insect, seldom found without the Tropics, being most numerous in the hottest Regions. Their offending Weapon is a Pair of *Forceps*, armed with two sharp Points, which meet when they bite, and cause a very acute Pain for eight or ten Hours, abating very gradually. Their Bite is said to be as bad as that of the Scorpion.

Hamamelis. This Winter flowering Shrub was sent to the Author from *Virginia*, and arrived at *Christmas*, full of yellowish Flowers.

3. *Monedula, tota nigra*. Hist. Jam. 298. Vol. 2. The Razor-bill'd Black-bird of *Jamaica*.

The singular Make of the Bill of this Bird distinguishes it from others; the upper Mandible being remarkably prominent, rising arch-wise, with a high and very thin Edge. They appear in numerous

Flights

Flights in *Jamaica* and *Hispaniola* voraciously destroying the Grain there, and in other warm Parts of *America*.

Calceolus, flore maximo rubente, purpureis venis notato; foliis amplis hirsutis crenatis; radice Dentis Canini.

This Plant produces the most elegant Flower of all the helleborine Tribe; and is in great Esteem with the *North American Indians* for decking their Hair, &c. They call it the *Mocasin* Flower, which also signifies in their Language a Shoe or Slipper, and is by these *Indians* made very like that ancient Slipper or Shoe lately found in the Isle of *Axholme* in *Yorkshire*, and shewn to the *Society Oct. 22. last* *.

4. *Vespa Ichneumon tripilis Pensylvaniensis.*

Rhus glabrum, Panicula speciosa coccinea. Pensylvanian Sumach.

This *Rhus*, for the Resplendency of its scarlet Panicles, excels all others of the Tribe. The Colour begins to appear in *July*, with a Tincture of Yellow; but as the Fruit ripens, the Scarlet heightens, as appeared by Plants in their full Lustre on the 30th Day of *September 1747*, in the Author's Garden at *Fulham*. The Berries that compose the Panicles were thick-set with numerous Filaments or small Threads of a purple or scarlet Colour (best discerned by a Microscope); which receiving a Reflexion from the Yellow, causes this glorious scarlet Colour, which nothing can excel; more especially when the Sun shines upon it. It is

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* See these *Transf.* N^o. 484, p. 575.

a Native of *Virginia*, but agrees well with our Climate.

5. *Pica luteo-nigra varia*. The yellow and black Pyc. *Hist. Jam.* p. 301.

These Birds in *Jamaica* are called *Bonano* Birds; that Fruit being a Part of their Food. They are very sprightly and active Birds, and are often kept in Cages, for their Docility, and antick Gestures.

Lilio-Narcissus Polyanthes, flore albo. This bulbous-rooted Plant grows plentifully in the boggy Soil of *Georgia*.

Vespa Ichneumon cærulea.

6. *Cacao Arbor*. The Cacao or *Chocolate* Tree.

This excellent Tree is found no-where but in *America*, and there only between the Tropics. 'Tis pity the Culture of this so useful and valuable a Tree should be neglected by us, when the Soil and Climate of all our Sugar Islands is as well adapted to its Growth, as any of the *Spanish* or *French* Territories. Notwithstanding which they supply us and all the World with it... Our Author thinks this deserves the Consideration of the Legislature; for were a Method found to encourage its Cultivation, we might not only supply our home Consumption of *Chocolate*, but come in for a Share of Exportation to foreign Markets.

7. *Volubilis filiquosa Mexicana, Plantaginis folio*. *Hist. Jam.* 180. Vol. I. The *Vanellœ*.

With the Fruit of this Plant the *Spaniards* perfume their *Chocolate*.

8. *Hirundo, cauda aculeata, Americana.* The American Swallow.

The Singularity of this Bird is, that the Shafts of the Tail Feathers are very stiff, sharp-pointed, and bare of Feathers at their Ends, which seem designed by Nature for the Support of their Bodies, while they are in an erect Posture, building their Nests; which they do in Chimnies, with little Sticks interwoven and cemented together with a kind of Glue or Gum.

This Bird arrives and retires from *Carolina* periodically, and agrees with the Description of *Murgravius's Andorinha* of *Brazil*; except that he takes no notice of the Spines in the Tail; which he might probably overlook. Could it be ascertained, that this and *Margrave's Andorinha* were the same, it would, I think, confirm that most probable Hypothesis, that Birds of Passage (particularly Swallows) pass in our Winter to the same Latitude in the Southern Hemisphere, as the Northern Latitude, from whence they came.

Lilium angustifolium, flore rubro singulari. The red *Pennsylvanian Lily*. This Lily comes from *Pennsylvania*. It agrees with our Climate.

9. *Pomifera, seu potius Prunifera Indica, nuceniformi, summo pomo innascente Cajous vel Acajous dicta.* *Raii Hist. Cat. Jam.* The *Cajou* or *Cassu Tree*.

This forms a regular-headed handsome Tree, producing beautiful fragrant Flowers succeeded by its wholesome and nutritious Fruit, which is also of singular Structure and Beauty: The Stem of the Fruit

is in a most singular manner placed at the Crown of the Fruit on the Outside of the Fruit itself, in Form of a Hare's Kidney, and contains a Kernel of the Size and Taste of an Almond: The Shell inclosing this Kernel is double, and contains an acrimonious caustic inflammable Oil; which, if applied to a tender Part of the Skin, fetches it off. It remains in Linen marked with it, the whole Time of wearing; and is therefore used for that Purpose in the *West Indies*.

10. *Ardea cristata maxima Americana*. The largest crested Heron.

This is the largest Species of Heron yet known, and is a great Devourer of Lizards, Efts, &c.

Stellio aquaticus minor Americanus. The spotted Eft.

Pulex minimus, cutem penetrans, Americanus. The *Chego*. This is smaller than the smallest of our common Fleas; they penetrate the Skin, under which they lay a Bunch or Bag of Eggs, which swell to the Bigness of a small Pea, and give great Pain till it is taken out: To perform which, great Care is required, for fear of breaking the Bag; which endangers a Mortification, and the Loss of a Leg, and sometimes Life itself.

Scarabeus capricornus minimus, cutem penetrans. This odd Insect I saw (says the Author) Governor *Phinney*, of the *Bahama Islands*, pick out of his Foot, as he was searching for *Chegoes*. It was larger than a common Flea; but magnified to 500 times its Size, appeared as here exhibited.

Blatta Americana. The Cock-roach. These are very troublesome and destructive Insects, and are so numerous and voracious, that it is impossible to keep Victuals of any kind from being devoured by them, without close covering. They are flat, and so thin, that few Chests or Boxes can exclude them: They eat not only Leather and Parchment, but Linen and Paper: They disappear in Winter, and are most numerous in the hottest Days in Summer, and commit their Depredations most at Night.

Blatta maxima fusca, peltata, non alata. So called from having a sort of Shield over their Head.

Scarabæus peltatus. This Beetle has its Name for the same Reason.

11. *Scarabæus pilularis, Americanus.* Tumble-turds.

This is the most numerous and the most remarkable of the Beetle-Kind of any in *North America*: The Employment they are always seen at, is providing *Nidi* for their Eggs; for which they are endowed with the like Sagacity as the Turkey-Buzzard*, to find out their Subsistence by the Excellency of their Noses, which directs them in Flights to Excrements just fallen from Man or Beast, on which they instantly drop, and fall unanimously to work, in forming round Pellets thereof, in the middle of which they lay an Egg. These Pellets in *September* they convey three Feet deep in the Earth, where they lie till the Approach of Spring, when the Eggs become animate, burst their Nests, and find their Way out of the Earth. Mr. *Catesby* says, he has

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attentively

* See this *Hist. of Carolina*, Vol. I. p. 6.

attentively admired their indefatigable Industry, and mutual assisting each other in rolling these globular Balls to the Place of their Interment. This they perform Breech foremost, by raising up their hind Part, and forcing along the Ball with their hind Feet. Two or three are sometimes engaged in trundling one Ball; which often meeting with Impediments by the Unevenness of the Ground, is deferred by them; yet by others is again attempted with Success; except it rolls into a deep Hollow or Chink, where they are necessitated to leave it; repeating the like Action with the next Ball that falls in their Way. No one seems to know his own Ball, but an equal Care for all seems to affect the whole Community. So intent are they at their Work, that tho' handled, or otherwise interrupted, they persist in their oeconomical Employment without Fear or Apprehension of Danger. The Size of this Insect is that of the Figure here exhibited; it is all over of a dusky Black; it has six Legs, two joined to the *Thorax*, and four to the *Abdomen*.

There are always accompanying these above-mention'd some larger ones, of a more elegant Structure and Colour, which are much less numerous, being about one in twenty to the other. The *Thorax* of this is cover'd with a Shield, of a crimson-colour'd metallic Lustre; the Head of the like Colour, blended with Green; on the Crown of the Head stands a shining black Horn, recurved backward, &c. These are commonly called King-Tumble-turds; tho', by what appears, they assume no Pre-eminence; but, without Distinction, partake of the like dirty Drudgery with the rest.

Lilium,

Lilium, five Martagon *Canadense*, floribus magis flavis non reflexis. The Canada Martagon. These Plants have flower'd several Years in Mr. Collinson's Garden at Peckham.

12. *Perdix sylvestris, Virginiana*. The American Partridge.

This is about half the Size of the *Perdix cinerea*, or common Partridge, but much more elegantly colour'd. These, contrary to ours, are not often found in open Fields, but mostly frequent Woods, and shady Swamps; their Flesh is remarkably white, and well-tasted, but of a different Flavour from ours. When raised, they perch on the Boughs of Trees.

Lilio-Narcissus, Virginienfis. Park. The Attamusco Lily. This Plant is a Native of Virginia and Carolina,

13. *Steuartia*. This elegant Shrub is so called in Compliment to the Earl of Bute, whose Family Name is *Steuart*. It flower'd in the Author's Garden at Fulham in May 1742. It is nearly akin to the Shrub-Mallows.

Regulus cristatus. The crested Wren. It is very remarkable, that this being the smallest of all our English Birds, is also found in America.

Vespa Ichneumon, of a yellowish brown Colour.

14. *Avis Tropicorum*. The Tropic Bird.

The Name of these Birds seems to imply the Limits of their Abode; for they are not often seen much North or South of the Tropics; yet are they seen all over the Ocean within those Limits, from

the Continent of the Old to the New World, and are very remarkable and different from all other Birds, in having a Tail consisting only of two very long narrow Feathers. The whole Bird is white, except the Bill and Legs and Feet, which are red, and about the Eyes, and near the Tips of the Wings are Spots of Black.

Larus minimus, marinus, naribus tubulatis. The Pittrel or Storm-Fink. This is a Sea-Bird, no bigger than a Sparrow, and is remarkable for being the smallest of all Birds that are web-footed. Their Appearance is generally believed by Mariners to prognosticate a Storm, or bad Weather. They use their Wings and Feet with surprizing Celerity: Tho' their Feet are formed for Swimming, they are likewise so for Running, which Use they seem to put them to; being ofteneft seen in the Action of running swiftly on the Surface of the Waves in their greatest Agitation, but with the Assistance of their Wings. The Author has seen them ofteneft in bad Weather.

15. *Magnolia, flore albo, folio majore acuminato haud albicante.*

This is the fourth and last-discovered Species of that elegant Tribe of Trees the *Magnolia*. Some Seeds of it with Specimens were sent me from the only Tree of it known in *Virginia*. Its majestic and elegant Appearance excites many People far and near to visit *Kit-Smith's Tree*; that being the Name it has attain'd, and is known from the Name of the Man in whose Land it grows. This Tree, tho' scarce in *Virginia*, has been since found to
grow

grow in great Plenty in the North-West Parts of *Pennsylvania*.

Formica villosa coccinea. The Velvet-Ant. This Insect is shaped like an Ant, and is about the Size of a Hornet. The Body is elegantly marked with black and crimson Velvet. The *Thorax* is of so strong and hard a Contexture, that, being trod upon by Men or Cattle, they receive no Harm. They have a long Sting in their Tails, which causes Inflammation and great Pain to those who are stung.

16. *Caprimulgus minor Americanus*. The Whip-poor-Will.

This nocturnal Bird is about the Size of a Black-bird. It has the smallest Bill, and widest Mouth, of any other Bird, in proportion to its Size; it hides itself in the Day, and is then never seen, but at the Dusk of the Evening he sets up his Cry, repeating it incessantly, till Break of Day, making a very loud and shrill Noise, which, the Echoes from the Rocks, and Sides of Mountains, increase to such a Degree, that the Silence of the Night is much interrupted thereby. Their Cry is like the Sound of the Pronunciation of the Words *Whip-poor-Will*.

The *Indians* say, these Birds were never known till a great Massacre was made of their Country Folks by the *English*; and that they are Souls or departed Spirits of the massacred *Indians*. Abundance of People look upon them as Birds of ill Omen, and are very melancholy, if one of them happens to light upon their House, or near their Door, and set up his Cry (as they will sometimes upon

upon the very Threshhold). Such are the Superstitions of these ignorant People.

Aureliana Canadensis. R. P. Lafiteau. The *Ginseng*, or *Ninsin* of the *Chinese*.

Ginseng is the Root of a Plant of the highest Esteem with the *Chinese* for its medicinal Virtues; and many Volumes have been wrote by their most celebrated Physicians, to illustrate its wonderful Effects. Tho' most of the Writers of *China* take notice of the *Ginseng*, yet it was little known, till Father *Jartoux*, a Jesuit and Missionary in *China*, who being employed by Order of the Emperor of *China* in making a Map of *Tartary* in the Year 1709. had an Opportunity of seeing it growing on the Confines of the Kingdom of *Corea*. That Father took an Opportunity to make a Draught of the Plant, and give an accurate Description thereof; which being published in the Memoirs of the *Academy of Sciences* at *Paris*, gave Light to the Discovery of the same Plant in *Canada* and *Pensilvania*; from which last Place it was sent to Mr. *Collinson*, in whose curious Garden at *Peckham* it has the preceding, and also this Year 1746, produced its Blossoms and Berries, as it appears in the Figure here exhibited, and agrees so exactly to the Father's Description of the *Chinese Ginseng*, that no Doubt can be made of its being the very Species he describes. But as the Jesuit's Account is too long to be inserted the Author has only given an Abstract of it, and added to his Figure the Blossoms which the Father owns he never saw.

This concludes the whole Number of Birds exhibited in both Volumes, containing in all

113; in which are also contained all the Land Birds Mr. *Catesby* ever saw or could discover in that Part of *North America* included between the 30th and 45th Degrees of Latitude. And tho' more Kinds may not improbably remain unknown within those Limits, yet North of them he thinks there cannot reasonably be thought to be many new Species; because there are not only but a few Birds at the Northern Limits, but also because Animals in general, and particularly Birds, diminish in Number of Species, as they approach the Pole.

17. *Chamaerhododendron Lauri folio, semper virescens, floribus bullatis corymbosis.*

This Tree riseth to the Height of about sixteen Feet, producing ever-green Leaves, in Shape like the *Lauro cerasus*, of a shining Dark-green; the Flowers grow in Bunches, the Bud or Rudiment of which appears in Autumn wrapped up in a conic scaly *Perranthium*, on which is a viscous Matter, which protects them from the Severity of the Cold in Winter. In the Spring these Buds break forth into monopetalous bluish-colour'd Flowers, with some of its Petals spotted with yellow, green, and purple. The whole Plant is of a most elegant Appearance: Its native Place is *Pensilvania*.

Chamaedaphne sempervirens, foliis oblongis angustis; foliorum fasciculis oppositis e foliorum alis. This Plant is a Native of *Pensilvania*, but has flower'd at *Peckham* in September 1743.

18. *Lepus Javensis*. The Java Hare.

It is about the Size of an ordinary Hare; the Head small, in proportion to the Body; the Eyes large and prominent; the Ears like those of a Rat; except which, the Head partook of a Likeness both of a Deer and a Hare; the hind Part of the Body remarkably big.

Ficus, Citri folio, fructu parvo purpureo. This is one of those kind of Trees mention'd by *Q. Curtius, lib. ix. c. 1.*

19. *Vipera marina*. The Viper-Mouth.

This Fish was 18 Inches long: But as Fish are not (as Quadrupeds) of a determinated Size, so these are said sometimes to grow to a vast Bigness. The Mouth was excessive wide: Both Jaws were armed with sharp destructive Teeth; particularly two in each Jaw were much longer than the rest, so that they could not be admitted within the Mouth. Most of these long Teeth had an angular bending towards their Ends in a very singular manner. It was without Scales, mark'd all over with hexagonal Divisions. This Fish was of the oddest Structure, and most formidable Appearance, of any the Author ever saw. It came from *Gibraltar*, and was taken in the Harbour there, and is now in Sir *Hans Sloane's Museum*.

Cataphractus Americanus. The Armour-Fish. This Fish was somewhat less than a Foot in Length, and four Inches broad; a small Part of the Belly was cartilaginous; except which the whole Fish was cover'd with hard thick Bone, but in a different manner; viz. the Head and fore Part of the Fish
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was also cover'd with Plates of Bone, extending from the Back to the Belly, and lapping one over another. It was armed with three strong pointed Bones, thick-set, or rather serrated with Teeth, one placed near the Back, and one near each Gill. These Bones were three Inches long, and so fixed in Sockets, that the Fish can point them to any Direction, in Defence of itself. This Fish having no Teeth for Defence, Nature seems to have compensated that Deficiency, by bestowing on him Weapons and Armour in a very extraordinary manner. It was given by Captain *Wm. Walker, F. R. S.* to Sir *Hans Sloane*.

20. *Bison Americanus*.

This is the only Species of the wild Cow-kind that is known in *North America*; there being none of our Cow-kind there, till brought over from *Europe*. They are low of Stature, but weigh more than our largest Oxen: The Skin of one is too heavy for the strongest Man to lift from the Ground: Their Limbs are large, their Chests broad, as are their Heads; their Horns are large at their Basis, and turn inward; on their Shoulders is a large Prominence or Bunch; in Winter their whole Body is covered with long shaggy Hair, which in Summer falls off, and the Skin appears black and wrinkled; except the Head, which retains the Hair on all the Year. On the Forehead of a Bull the Hair is so long, that, by hanging over his Eyes, it impedes his Flight, and is frequently the Cause of his Destruction. But this Obstruction of Sight is in some measure supplied by his good Nose, which is no small Safeguard to
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him.

him. A Bull in Summer, with his Body bare, and his Head muffled with long Hair, makes a very formidable Appearance. They range in Drove, feeding in open *Savanna's* Morning and Evening; and in the sultry Time of the Day they retire to shady Rivulets, and Streams of clear Water, gliding through Thickets of tall Canes; which, tho' a hidden Retreat, yet their heavy Bodies causing a deep Impression of their Feet in moist Land, they are often traced and shot by the artful *Indians*. When wounded, they are very furious; which cautions the *Indians* how they attack them in open *Savanna's*, where no Trees are to screen them from their Fury. Their Hoofs more than their Horns are their offensive Weapons; and whatever opposes them are in no small Danger of being trampled into the Earth. Their Flesh is very good, of a high Flavour, and differs from common Beef, as Venison from Mutton. The Bunch on their Shoulders is esteemed by the *Indians* the most delicate Part of them.

Pseudo-Acacia hispida, floribus roseis. The Flowers and Leaves of this Tree differ little in their Shape from the *Pseudo-Acacia flore albo*. The Stalks and larger Branches are thick-set with prickly Hairs, and with sharp Spines, placed alternately: The Flowers, which are papilionaceous, are of a faint purple or rose Colour, and of a fragrant Smell. I never saw any of these Trees but at one Place near the *Apalatchian* Mountains, where Buffaloes had left their Dung, and had been brouzing on the Leaves. What with the bright Verdure of the Leaves, and the Beauty of the Flowers, few Trees make a more elegant Appearance.

Thus

Thus ends the most magnificent Work I know of, since the Art of Printing has been discover'd: The Descriptions are all given in both *English* and *French*; and the Figures being drawn by the ingenious Author after Life, were afterwards etched by himself, and all the illuminated Sets were colour'd under his Directions, and all touch'd up and finish'd by his own Hand.

VII. *The Inscription upon a Roman Altar found near Stanhope in the Bishoprick of Durham; communicated to the Royal Society by the Reverend Mr. Thomas Birch, F. R. S.*

Read Feb. 18.
1747-8.

SILVANO INVICTO SACRVM
C TETIVSVETVRIVSMICIA
NVSPREFAIAESEBOSIAA:
NAEOBAPRAMEXIMIAE
FORMAECAPTVMQVEM
MVLTIANTECESSO
RESEIVSPRAEDARI
NONPOTVERVNTVSLP

Silvano invicto sacrum
C. Tetius Veturius Micia-
nus, Præf. Alæ Sebosi-
anæ, ob Aprum eximie
formæ captum quem
multi Antecesso-
res ejus prædari
non potuerunt Votum solvens libens
posuit.

VIII. *A Letter from Mr. Henry Baker, F. R. S. to the President, concerning an extraordinary Fish, called in Russia QUAB; and concerning the Stones call'd Crabs-Eyes.*

Read Feb. 25.
1747-8.

HAVING been invited some time ago to a Correspondence in *Muscovy*, with Dr. *James Mounsey*, one of the Physicians to the *Czarina's* Armies, a Gentleman much esteemed in that Country, for his Knowledge in Natural Philosophy, and his unwearied Endeavours to discover Truth, I readily embraced so favourable an Opportunity of making Inquiry concerning some Things, as to which the Accounts hitherto received appeared to me extremely doubtful.

I therefore desired of the Doctor to send me what Information he could depend on, first, concerning the Swallows; and other Birds of Passage, that are observed in *Russia*, as we have had some Accounts of them that seem incredible. Secondly, concerning an extraordinary Fish in that Country, called the *Quab*, which is reported to be first a Tadpole, then a Frog, and at last a Fish. And, thirdly, I requested of him to inform me concerning the *Oculi Cancrorum*, commonly called *Crabs Eyes*, particularly as to their Production, and the Manner of their being gather'd.

In Answer to these Inquiries, I was favoured by the Doctor with a most obliging Letter, accompanied

nied by that remarkable Case of a *Fœtus* extracted by him from one of the *Fallopian* Tubes, where it had been lodged 13 Years (see this *Transf.* p. 131.). And I now beg Leave to lay before you some Extracts from this Letter, as Matters not only of Curiosity, but of considerable Moment also, in the Natural History of Animals.

He desires I'll allow him another Year to perfect his Observations on the Birds of Passage; being unwilling, he says, to rely on the Accounts of others, where he can come at the Knowledge of Things himself.

As to the *Quab*, which some report to be first a Tadpole, then a Frog, and at last a Fish, 'tis very well known, he says, to him; but with regard to such Changes, he believes them to be intirely fabulous. He has indeed seen, in the Chamber of Rarities at *Petersburg*, this Fish, preserv'd in Spirits, under all these Appearances; but was not permitted to take out any one of them, in order to remove the Scruples he made: However, desiring as far as possible to come at the Truth, he turn'd the Bottle hastily on one Side, to make the Fish fall to the Glass, which he thought they did, with more seeming Hardness than could be supposed in Fishes; which induced him to conjecture, that they are Pieces of Art, the Idea whereof has been taken from the Resemblance of the Head of this Fish to that of a Frog. Whence he supposes they may be made of Wax, and kept in this manner to amuse the World. If there be, he says, such a Thing in Nature (which he does not think probable) it must be peculiar to some one Place, whereof he has no

Knowledge. He has made Inquiries about these imagin'd Changes, of People of many Nations, but could never learn any thing to the Purpose. He has seen the Fish itself in several Countries, and found they spawn'd like other Fishes, and grew in Size, without the least Similitude to what has been asserted. He adds further, that these Fishes delight in very clear Water, in Rivers with stony or sandy Bottoms, and are never found in standing Lakes, or Rivers passing thro' marshy or mossy Grounds, where Frogs chuse most to be.

As to my Inquiries concerning the *Crabs-Eyes*, he expresses a Surprize to find Naturalists differ so much from one another, and yet not one of them he has ever seen giving any true Account of the Situation, Formation, and casting of these concreted Bodies. He therefore is so obliging to send me the following Description from his own Observation and Knowledge.

Those Concretions called *Crabs-Eyes*, are found, says he, in the Bodies of Cray-fish. Each Fish annually produces two, one on either Side of the anterior and inferior Part of the Stomach, and each is generated about a Point lying between the Coats thereof. The flat or concave Side lies next the internal Coat, which is very thin and clear, though strong and horny; the convex Side is consequently outwards, and is immediately cover'd by the fleshy and softer Coats of the Stomach, whose Fibres make Impressions on its Surface. Between these two Membranes it grows by degrees *lamellatim*, and is supplied with petrifying Juices discharg'd through the Mouths of Vessels or *Sudamina* opening on the internal

nal Surface of the outer Coat. The inner Membrane, being horny, gives Resistance only; wherefore the Stones are concave on that Side, and the first remarkable Scale (whereon all the others are formed) may be perceiv'd in the Centre, the Brims or Circumferences of many of the rest being very apparent. At the time these Stones are not to be found in the Animal, there are little circular Spots, somewhat opake, and whiter than the rest of the Stomach, to be perceiv'd in their Place; nearly opposite to which are tenacious mucilaginous Substances, form'd like little *Placentulae*, and call'd by some the Glands of the Brain: These are larger, and more perceptible when the Stones are wanting; but are not turned into Stones by different Degrees of Induration, as some have imagined them to be.

It is believed, he says, that they cast these Stones with their Shells, which they shed every Spring; but he finds this is not the Way of getting rid of them; for, a little before, or after the Time of their casting their Shell, the Stones break thro' the internal or horny Coat of the Stomach, and being ground or broken by the three serrated Teeth therein, become dissolv'd in the Space of a few Days, which makes it difficult to find them just at this time, and thereby gives Ground to imagine they are cast with the Shells. He says, however, he has found several of them in the Stomach partly consumed, one Specimen whereof he has sent herewith, and a farther Proof that they are so consumed, is, he thinks, their being never discover'd in Rivers, tho' the Fish themselves be in great Plenty there; and in the Shops it is observable, that many of these Stones are of a
brown

brown Hue ; which is the Case of such as have been already lodg'd in the Cavity of the Stomach, when the Fish was taken. They likewise eat the old Shells immediately after shedding them *. What the Use of these Stones to the Creature is, he cannot positively determine, but supposes they may be design'd to furnish new petrescent Juices to its Fluids; which may be also assisted by the old Shells which they devour, the Particles whereof, as well as of the Stones are probably dispos'd of, according to their Degree of Purity, and properly deposited at the Extremities of Vessels, for the Reproduction of their annually new crusty Dress; which, he observes, does not greatly recommend the Opinion that these Stones have a dissolving Quality, of Service against the Stone in the human Kidneys or Bladder.

The Doctor has sent along with this particular Account, Specimens of the Cray-fish both boil'd and raw, which differ little or nothing from those catch'd in our Rivers here; in which I am assured the like Concretions may be also found at a certain Time of the Year: He has likewise sent me some of their Stomachs dried, where the Stones appear, situated in the manner above described between the two Coats; and in one of them they are got thro' the internal Coat into the Stomach itself. I received also from him several Specimens of the beginning Scales,

* I have observed the same thing in the small fresh Water Shrimp; which I have kept in a Glass with Water throughout several of the Periods of its casting its Shell, which it does once in about a Month or five Weeks. The Water Newt also eats its Skin as soon as pull'd off, if it be not taken away.

Scales, or Concretions, of different Bigness, which he collected himself, in dissecting these Creatures; several of the formed Stones of his own taking out, some of a larger Size, which were given him by a Gentleman, who took them out of the Cray-fish in the River *Donne*, and others still larger, which he chose from the Apothecary of the Army. These last were from *Astracan*; and he observes that the Fish and Stones are much the largest in the great Rivers there, where there are Fishers for Cray-fish on account of the Stones only; which they separate from the Fish at different Fisheries after different Manners; at some they are beaten to Pieces with wooden Pestles; then washing away the Flesh and Shells, the Stones are found remaining at the Bottom of the Vessel; at others they are laid in Heaps till they rot; and then, being wash'd, the Stones are easily separated and gather'd. The Price comes to a Groat or Sixpence a Pound. All the Apothecaries Shops throughout the whole *Russian* Empire are furnished with them, and great Quantities besides are exported.

This, Sir, seems to be a very particular and exact Account of these Productions, which are frequently prescribed in Medicine. Their Price, we find, is extremely low in the Countries where they are gather'd; notwithstanding which, fictitious Bodies, made of Chalk, Tobacco-pipe Clay, or other such-like Materials, cast in Moulds, so as to represent

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real Crabs-Eyes, are often substituted instead thereof. Permit me to subscribe myself with all possible Respect,

S I R,

London, Feb. 25.

1747-8.

Your most obedient humble Servant,

Henry Baker.

IX. Brevis historia naturalis, five de Vita, Genere, Moribusque *Muris Alpini*: Autore *Jacobo Theodoro Klein*, Reipubl. *Gedanens.* à Secretis, et R. S. *Lond.* S. communicata per *Petrum Collinsonum*, ejusdem Societatis S.

Read Feb. 25.
1747-8.

BESTIÆ ex murium gliriumve genere ad *animalia industria* numerantur. Nulli bestiarum, nedum insectorum multitudinibus, ingenitam denegamus industriam, *i. e.* mores et studia, pro vitæ suæ genere, ad conservationem et progeniem adæquata. In quibusdam circumscripta nobis videntur; uti juxta *aranearum* tribum, five ex telis, staminibus, retibus vel cassiculis scientifice constructis, five ex latebris victum capientium; quædam *philomusos* dixeris, ut admirandum animal *Castorem* (a), ligationis, hydrographiæ, aggerationis et architecturæ

(a) Conf. *quadr. Hist. prodrom.* p. 19. --- *Hist. de l'Acad. des Scienc.* 1737. p. 10. *ibid.* On ne trouve guere parmi les grands Animaux, que les *Castors*, qui ayent une de ces industries singulieres et incomprehensibles a l'Esprit humain.

recturæ studiosum; nonnulla pharmaceuticam et geometricam artem edocta, ut *Apes*; rursus alia ad artes mere liberales: Petauristæ, ferrarii, naviculatores (*b*) ut *Sciuri*, se profitentia; ne dicamus de insidiatoribus, prædatoribus, ut *Baviis*, et sicariis, latrocinia et abactionem exercentibus, *Urso*, *Lupo*, *Vulpe*, *Mustelis*; sic nemo facile animalibus *industriam*, pro vitæ suæ genere, abjudicaturus.

Sed valde fallimur, fallimusque aliquando, circa mores nonnullorum, quos ingenitæ industriæ superaddimus, multoties cerebrinos. Errores nobis ipsi tribuamus, quod sæpissime historiolas ex antiquitate ad nos transfusas æqui bonique habemus, quæ vero ad rectæ rationis examen revocatæ in fabulas abeunt.

Ecquis, emuncti nasi, cum mundo symbolico crederet, Lernam catulos edere inanimatos, rugitu matris vel patris animandos? Quis informem massam sive molam *Ursi*, lambendo a matre in sui similem bestiam efformandam? Quis *angues*, quos astutia et malitia omnes bestias antecellere dicimus, e cavernis, narium cervi spiritu evocandos, ut se devorari patiantur? Quis fide dignus testis centesimum *Cervi* superavit annum, quo hujus lacryma ipsa ossibus ad oculi canthum accreverit ea duritie, quæ cornu superaret? Quis cum Sophocle *lacrymas Gallo-Pavonis* vel *Meleagridis* (*c*) verteret in *Succinum*? Et sexcenta commenta reliqua.

Quod

(*b*) *Phil. Transf.* No. 427. p. 38.

(*c*) Conf. *Franc. Vetez. Hist.* de los animales, p. 70. *Sophocles* entre los antiguos, dixo, que et *Succino* era *lagrimas congeladas* de unas aves llamadas *μελαγχρίδης* de los Griegos, las quales son un genero de Gallinas de las Indias o Moriscas.

Quod ad MURES ALPINOS attinet; nomen habent a summis *Rhetiae* Alpibus, herbarum graminisque fertilibus, in quibus habitant. In Sabaudia audiunt *Marmota*, in Germania *Murmel Thiere*, in Ukraina, Podolia in montibus Scepusiensibus, necnon in Russia Palatinatu ad pagos Podicemne et Zimnawoda *Bobaszi* (d) ad montes vero Carpaticos *Swiszez*, unde vallis Swiszeza denominatur, Gallis, *Rats des Alpes*.

Fele domestica corpulentiores sunt; *juniores* rufi; *adulti* subfusci coloris; *pilis* rigidiusculis; *pedibus* brevioribus; *capite* contracto; *naso* quasi diviso; *ore* felinis pilis stipato; *dentibus* sciuri, vel potius fiberinis; *collo dorsoque* latis et obesis; *auribus* decurtatis, quasi mutilis, rotundis; *oculis* speciosis, prominentibus; *cauda* adinstar caudæ muris avellani, quasi compressa et pilosa, palmam cum dimidio longa; *digitis unguibusque* sciuri æmulis, sed fortioribus.

In *libertate* fructibus, herbis, gramine, radicibus, imo et insectis vaginipennibus ac locustis vescuntur; *cicurati* variis obsoniis, inprimis lacteariis (e) delectantur; carnes, panem, fructus, similemque pasum, ut sciuri, anterioribus pedibus ori admoventes.

Catulos ponunt tres vel quatuor; ab *autumno* in verna usque tempora, per familias juncti, in cavernis ad figuram literæ Y excavatis, et bene obturatis, satis ample fornicatis, *alto somno* inter tantillum straminum vel foeni sepulti quiescunt; sole rurius propitio

(d) *Rzaczynski*, in auctario Hist. Nat. *Polon.* p. 327. quod nondum publice proffat.

(e) Murmuratum edunt, dum morfu bibunt lac: Unde *Germanis*, me iudice, *Murmel Thiere*; i. e. *murmurans Bestia*.

pitio *evigilant*, vigilantque claustra solventes, et ad *egestionem* fœcum, ad *pastum*, et ad opera *aphrodisiaca* exeuntes *colludunt* petulanter, uti solent *mures* inprimis campestris, *saltantes* et in arborum *truncos* ludibundi *scandentes*; aliquando *bipedes* ingrediuntur; vocem cælli vel acutum fistulæ sonum edentes.

Tardissima ipsis est per hyemem *sanguinis circulatio*; tardissimæ omnes sunt corporis *secretiones*; *nulla* interim *seri* vel lymphæ *resectio*, ut tandem sanguis omni pene sero orbetur (f). *Omentum* et vicina *interanea* admodum sunt obesa. *Ruminantes* non dixeris; *ventriculum* enim simplicem habent, membranosum, licet in libertate constituti herbivori sint; versus intestinum *cæcum* *valvulas* observaveris *convrutes*, *annulares*, et veluti in ramos protensas, prout ingressus *Ilei* inter duas tunicas annulares; ita, ut *nullus* prorsus concedi possit *excrementis*, pro *egestione*, ad intestina tenuia *regressus*, quorum tamen collectio per integram hyemem (qua dormiunt) fit, quæ amandantur ad *cæcum*, ibique ad verum usque tempus perdurant.

Hoc vitæ genus est, illi sunt mores, quos exploratos habemus *Murium Alpinorum*, quî BOBAKI vocantur. Quid per *Emptram*, murem montanum, intelligant *Albertus* et *Agricola*, me latet; cum mure autem Norvagico, *Lemmus*, *Leming*, *Lommer* dicto, *Mures Alpini* nihil commune habent; illôs
Wormius

(f) *Philos. Transf.* No. 397.

Wormius (g) in nubibus generari vult, exinde in Norvagiā deciduos, qui tamen et in Lapponia sunt frequentes.

Moribus Marmotarum superaddidit *Plinius*, sed notanter *ex relatione aliorum*, prorsus singularia: —
 “ Sed si pabulo ante in specus convecto, cum quidam
 “ narrent, alternos marē et fēminā supra se com-
 “ plexo fasce herbæ supinos, cauda mordicus appre-
 “ hēsa, invicem detrahi in specum, ideoque illo
 “ tempore detricto esse dorso.” Hanc narrationem
 vertit *Agricola* in historiam Marmotæ naturalem:
 “ Mira verō, inquit, eis machinatio et solertia, cum
 “ fœnum jam congesserunt; unus enim humi strā-
 “ tus erectis pedibus omnibus jacet in dorso, in quem
 “ tanquam in plaustrum quoddam cæteri ea, quæ
 “ congesserant, coniciunt, et sic onustum cauda
 “ mordicus comprehensa in specum trahunt, et quasi
 “ quodammodo invehant.” Cui *Gesnerus* non vi-
 detur assentire, inquit: Eodem modo Castorem
 ligna advehere, et *Taxos* effossam terram pro domi-
 ciliis, donec satis ampla fiat, vehere, apud recen-
 tiores legimus (b). Fides penes autores esto: Facetiam
 quoque

(g) In museo suo — et in peculiari tractatu, cui titulus: *Historia animalis, quod in Norvagia quandoque e nubibus decidit. Hafnia 1653, 4to.* Conf. *Scheffer*, *Lapponia*, c. 29. *Hamb. Beytr.* 1741. X. 28. *Lin.* act. *Suec.* et *Fauna Suec.*

(b) Notum potius testatumque facimus: *Castores* xyloromos, non per terram farraco, sed per flumina, infixis in ligna ad littora cæsa dentibus suis, lignationes pro palatiis advehere. *Rzaczyński* p. m. olim hac de re me conveniens responsum tulit: Noli figmenta comprobare. Fuit apud nos vir militaris strenuus venator, qui omni animo, invocatis cunctis divis, nobis persuadere volebat, quod *Vulpes* terram in cryptis inversæ caudæ imponat, eamque retro gradus extra antrum, außerhalb dem Bau, deponat. Sed fabula manet.

quoque hominis industrii et fictum appellare *idem*. Solere hos mures foenum in dorso gestare, cauda per dorsum reducta, et mordicus apprehensa (i) obfirmatum, pro fune scilicet, ut homines foenum bajulant.

Miramur, doctissimum *Spon* suprafatam narrationem ex animo pro vera venditasse historia: “ Rats
“ des Alpes faisant *provision l'été pour l'hiver* du
“ foin et d'autres herbes, qui leur sont nécessaires,
“ pour s'en acquiter plus promptement, il y en a un,
“ qui sert de *Charrette*, se mettant sur le dos, les
“ pattes en l'air, et embrassans le foin. et un autre,
“ qui sert de *Charretier*, et le tire par la queue
“ jusqu' à leur tannière; ce qui est cause, qu' on leur
“ ordinairement le dos tout pelé (k).

Magis miramur novissimam relationem, *magni*, et post fata, *nominis* CARDINALIS POLINIACI Mures Alpinos bella inter se gerere, trucidare se invicem beligerantes, et victores victos secum ducere in servitutum ad sustinenda servitia domestica, et pro invehendis rebus ad victum necessariis (l) mores ante a nemine observatos; quos si *Eminentissimus Purpuratus*

(i) Cum tamen muribus *Alpinis* sit caudæ nimis curta supellex.

(k) Et foenum et herbæ recentes in antris putredinem hyeme contraherent. Cæterum obesitas dorso, petulantia et somnolentia raritatem pilorum in dorso marmotis, et feris et cicuratis, conciliant. Nonne et alia quadrupedia (fere omnia pilosa) capillorum defluvia patiuntur?

(l) In *Antilucetio*, lib. vi. vers. 185. seq. (*Paris*. 1747, 8vo.)

(*Baubaces* patria dixerunt voce Poloni)

Vulpinum genus —

— de latebris et gramine certant.

— In captivos dominorum provida miras

Sævitia exercet pænas, mœsta agmina condunt

In foveis, coguntque omnes servire per annos :

Atque

ratus ipse suspectos non habuit, nobis imperaverit :
obtinere jus belli et pacis inter bestias sui generis ;
cum tamen

Cornix cornici nunquam perfodit ocellum.
nec lupus lupum est ; et quem in finem *mancipia*
Marmotis pro victu colligendo necessaria ? Quod certo
certius novimus, Mures Alpinos vel per *octo menses*
dormire, nullis obsoniis opus habentes ; nec ullum
mancipiorum negotium, quasi per tractatus *d' Affiento*,
inter illos et alias bestias intercedere potest, nisi forte
ipsis abuterentur, prout homines mancipiis auro so-
lido comparatis jure utuntur ad fodiendam terram.

Quicquid autem sit juxta novellam historiam ; juxta
antiquam fabulam ridiculum audit ; *equis ad poste-*
riorem currus partem uti, et inverso ordine naturæ,
contra naturalem, pilorum et exuviarum dispositio-
nem, mordicus prehensa, temonis loco, cauda anima-
lis, idem animal, quasi maleficum et damnatum,
veluti ad currum vel traham religatum trahere, inque
ipsa via vecturæ adferre obstacula. Si pro lecto opus
habent mures foeno, sufficit ejus parca collectio et
transportatio vel ore vel pedibus anterioribus medianti-
bus, quod et bipedes ingredi posse bestiolas novimus.

Sic est : *Historia naturalis* non bene digesta abit in
fabulam ; præjudicia vero et nimia credulitas *Verita-*
tem, etsi cominus satis cognitam, longissime ali-
quando propellunt. Hoc est.

X.

Atque ubi tempestas bruma veniente rigeſcit,
Et complenda manet ſectæ cellaria foeno,
Protinus ad meſſem ducunt ſervata ſerendam
Mancipia, inverſiſque ſolum premere atque ſupinis
Corporibus, tum crura jubent attollere ſuſum,
Quatuor erectis perſiſtent ut gramina palis.
Inde onerant caudaque trahunt animantia plauſtra:
Eraſque vias miſerorum tergore verrunt.

X. *Part of a Letter from Abbè Nollet, of the Royal Academy of Sciences at Paris, and F. R. S. to Martin Folkes Esq; President of the same, concerning Electricity.*

*Translated from the French, by T. Stack, M. D.
F. R. S.*

S I R,

*Read Feb. 11.
1747-8.*

FOR several Years past Electricity has been my chief Occupation. Last Summer I read three Memoirs at our weekly Meetings, which contained many Particulars on this Subject: But as these were Matters of mere Curiosity, and of no real Use, they almost tired out my Patience. I now send you some Experiments, which I made during the Vacation, which seem to promise at least the being of some Service; but of this you will be the best Judge. I will describe them in the same Order as I made them, and to which I was not led by mere Accident. You know, that when a Vessel full of Liquor, which runs out through a Pipe, is electrified, the electrified Jet or Stream is thrown farther than usual, and is diverged into several divergent Rays, much in the same manner as the Water poured out from a watering Pot. Every body at first Sight will judge, that the Stream is accelerated, and that the electrified Vessel will soon be empty. I was unwilling to rely on the first Appearances, and therefore resolved to ascertain the Fact, by measuring the Time, and the Quantity of the Liquor running out.

B b

And

And in order to know if the Acceleration, supposing there was any, was uniform, during the whole Time of the running out, I made use of Vessels of different Capacities, terminating in Pipes of different Bores, from three Lines Diameter to the smallest Capillaries: And I give you in gross the Result of upwards of an hundred Experiments, as it is not so easy a Task to draw a safe Conclusion, as may at first be imagined.

1. The electrified Stream, tho' it divides, and carries the Liquid farther, is neither accelerated nor retarded sensibly, when the Pipe, thro' which it issues, is not less than a Line in Diameter.
2. Under this Diameter, if the Tube is wide enough to let the Liquid run in a continued Stream; the Electricity accelerates it a little, but less than a Person would believe, if he judged by the Number of Jets that are formed, and by the Distance to which it shoots.
3. If the Tube is a capillary one, from which the Water ought naturally to flow, but only Drop by Drop, the electrified Jet not only becomes continued and divided into several, but is also considerably accelerated; and the smaller the capillary Tube is, the greater in proportion is this Acceleration.
4. And so great is the Effect of the electrical Virtue, that it drives the Liquid out of a very small capillary Tube, thro' which it had not before the Force to pass, and enables it to run out in Cases, where there would not otherwise have been any Discharge.

These

These last Facts have served as a Basis to my Inquiries. I consider'd all organized Bodies as Assemblages of capillary Tubes, filled with a Fluid that tends to run thro' them, and often to issue out of them. In consequence of this Idea, I imagined, that the electrical Virtue might possibly communicate some Motion to the Sap of Vegetables, and also augment the insensible Perspiration of Animals. I began, by some Experiments; the Result of which confirm'd my Notions. I electrified, for four or five Hours together, Fruits, green Plants, and Sponges dipp'd in Water, which I had carefully weigh'd; and I found, that, after this Experiment; all these Bodies were remarkably lighter than others of the same kind, weigh'd with them, both before and after the Experiment, and kept in the same Place and Temper. I also electrified Liquors of all sorts in open Vessels; and I remarked, that the Electrification augmented their Evaporation, in some more, in others less, according to their different Natures. Wherefore I took two Garden-Pots, filled with the same Earth, and sowed with the same Seeds; I kept them constantly in the same Place, and took the same Care of them, except that one of the two was electrified for fifteen Days running, for two or three, and sometimes four Hours a Day. This Pot always shewed its Seeds raised two or three Days sooner than the other, a greater Number of Shoots, and those longer, in a given Time: Which makes me believe, that the electrical Virtue helps to open and display the Germs, and facilitates the Growth of Plants. I advance this, however, only as a Con-

jecture, which deserves further Confirmation; as the Season was already too far advanced, to allow me to make as many Experiments as I could have wish'd: But here are yet other Facts, of which I have a greater Certainty, and which are not less interesting.

I chose several Pairs of Animals of different kinds, Cats, Pigeons, Chaffinches, Sparrows, &c. I put them all into separate wooden Cages, and then weighed them. I electrified one of each Pair for five or six Hours together: Then I weighed them again. The Cat was commonly 65 or 70 Grains lighter than the other; the Pigeon from 35 to 38 Grains; the Chaffinch and Sparrow 6 or 7 Grains: And in order to have nothing to charge upon the Difference that might arise from the Temperament of the Individual, I again repeated the same Experiments, by electrifying that Animal of each Pair, which had not been electrified before; and notwithstanding some small Varieties which happen'd, the electrified Animal was constantly lighter than the other in proportion.

Electricity therefore increases the insensible Perspiration of Animals: But in what Proportion? In the *Ratio* of their Bulks, or in that of their Surfaces? Neither of the one or the other, strictly speaking, but in a *Ratio* much more approaching to the latter than to the former. So that there is no Room to apprehend that a human Person electrified would lose near a 50th Part of his Weight, as it appear'd to me that it happen'd to one sort of Bird; nor the 140th Part, as to the Pigeon, &c. All that I have been hitherto able to learn upon this Head,

is,

is, that a young Man or Woman, from 20 to 30, being electrified during five Hours, lost several Ounces of their Weight, more than they were wont to lose, when they were not electrified. These last Experiments are difficult to pursue with Exactness; because the Cloathing, which cannot strictly be compared to the Hair or Feathers of Animals, retains a good Share of the perspired Matter, and hinders one from forming a good Judgment of the whole Effect of the electrical Virtue.

This forced electric Perspiration is very naturally accounted for, if we consider, that the electrical Matter pervades the interior Parts of Bodies, and that it visibly darts from within outward: For it is very plain, that these electrical Emanations must carry with them whatever they find in the small Vessels, thro' which they are seen, or at least are known, to issue.

This Explanation will, in my Opinion, occur to every one, who has seen the principal *Phænomena* of Electricity. But how shall we account for all the following Effects? All those Animals, whose Perspiration is increased upon their being electrified, all those Seeds, which shoot and grow quicker; all those Liquors, which evaporate; all that Acceleration of Liquids flowing thro' Tubes; all those Particulars, I say, happen in the same manner, when, instead of electrifying those Bodies themselves, they are only held near electrical Bodies of a pretty large Bulk. The Notion which I have, for these three Years past, formed of Electricity, not only affords me an Explication of this, as simple as the former, but I venture to say, it was this same Notion, that led

led me to the Experiments, and made me even foresee their Success.

I am not only satisfied of the Existence of an *effluent* electric Matter, which all the World allows, and which shews itself a thousand Ways; but many convincing Reasons have also assured me, that there is, round every electrified Body, an *affluent* Matter, which comes to it not only from the ambient Air, but likewise from all the other Bodies, whether solid or fluid, that are round about, and within a certain Distance of it. If these surrounding Bodies are of a simple Nature, as a Stone, a Piece of Iron, &c. nothing issues from them but pure electrical Matter: But if they are Animals, Plants, or Fruits, or, in a Word, any organized Bodies, or such, in the Pores of which there is any Substance capable of giving way to the Impulses of the electric Matter; this Matter will, in issuing forth with the great Rapidity, which it is known to have, carry along with it whatever it finds moveable enough to be displaced by it; and by so much will the Weight of the Body be diminished; the same Effect being here produced by the *affluent* Matter, as is produced on electrified Bodies by the *effluent*. If you will please to read over my Essay, what I advance will be better understood. The Increase or Diminution of Perspiration is not a Matter of Indifference to the animal Oeconomy: This new Method of increasing it at Will may possibly prove of Use; it is neither inconvenient nor dangerous; and neither I myself, nor any body else of those on whom I made my Experiments, suffered even the least Inconveniency from it. One feels neither Motion nor Heat differing from that of the natural State.

State. Nor did the Animals give any Signs of Uneasiness, while they were electrifying: A little Weariness, and a better Appetite, were the only Effects we ever perceived.

As to the Facility of applying this Method, 'tis well known that the electrical Virtue is easily transmitted a good way off by Chains, &c. ; and one may easily imagine, that an easy Chair, or even a Bed, suspended or supported in a proper manner, will put the most infirm Persons in a Situation to be very commodiously electrified. But as there is no Necessity to electrify them actually, it will become easier still; for nothing more will be requisite, than to place near them a Basket of old Iron render'd electrical, The commonest Degree of Sagacity will suffice to put this Method in Practice, whenever it is found to be useful.

I shall observe further, that, when I electrify an Animal, I render his Perspiration more copious; and this Effect is universal thro' every Part of it. When I only place it near an electrified Body, it perspires as much. But is its whole Body equally sensible of this Effect? I mean, what exhales in consequence of the Electricity, does it issue from every Part of his Surface? I believe it does not; and that for these Reasons.

If it be the electrical Matter of the Skin that drives out the Matter of Perspiration, by rushing towards the electrified Body, it is natural to think, that this Effect takes place only in the Part out of which the electrical Matter issues: Thus the Perspiration, which is electrically forced out, ought to issue from those Parts only, which are the most directly applied toward the electrical Body. Let us confirm this by Experiments.

To

To an electrified Body I apply a Vessel full of Liquor, which issues Drop by Drop thro' several little Tubes placed in different Parts of its Circumference : These Drops become continued Streams, and are accelerated, as if the Vessel had been electrified : But this Effect is observable on that Side only which faces the electrified Body.

I moisten a thick Sponge with Water, and cut it in two : I weigh these two Halves separately, I join them again, and place the whole near a large electrified Body, so as to make one Half of the Sponge face the Body directly, and the other the contrary Way. After an Electrification of five or six Hours, that Half, which faced the electric Body, was found to be lighter than the other, &c.

Wherefore I think I have good Grounds to believe, that a Man, who presents a Shoulder, or one Side of his Head, to a large electrified Body, perspires more thro' that Part than thro' any other. Add to this, that since these Animals, which I caused to perspire in this last manner, and which had but one Side of their Bodies exposed to the Electricity, lost as much of their Weight, as the others which were thoroughly electrified; it follows, that they perspired as plentifully thro' the exposed Part, as the others thro' the whole Body. Whence we may infer, that, of the two Methods, which I propose for augmenting insensible Perspiration, the latter is the most powerful, and most proper to remove Obstructions from the Pores, or to scour them of any noxious Humours which they may happen to contain. I have the Honour to be, with the greatest, Respect, Sir,

Your most humble and most obedient Servant,

The Abbè Nollet.

XI. *Several Effays towards discovering the Laws of Electricity, communicated to the Royal Society by Mr. John Ellicott F.R.S. and read on the 25th of Feb. 1747. and at two Meetings soon after.*

I. *A Letter to Martin Folkes Esq; Pr. R. S.*

S I R,

Read Feb. 25. 1747-8. **I**N the * Letter lately read before the

Royal Society from the Abbè Nolet, containing his Observations on the Increase of the Transpiration of Animals, and the Growth of Vegetables, by means of the electrical *Effluvia*, he takes notice, that he was led to those Inquiries, from the Acceleration which (he found from a great Number of Experiments) was given to the Motion of Fluids thro' capillary Tubes, upon their being electrified. As I formerly made several Experiments on this Subject, I shall submit it to your Consideration, whether the following Observations on those Experiments may deserve the Notice of this illustrious *Society*. In which I have principally endeavoured to prove, that the Acceleration of the Motion of Fluids thro' capillary Tubes or Syphons is not barely owing to their being electrified, but that, in all Cases whatsoever, there are some other Circumstances necessary, in order to produce this Effect. And I doubt not but to make this fully appear, by shewing, that Water, being electrified, may either be made to run

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in

* See the preceding Paper, *Art. X. p. 187.*

in a constant Stream thro' a capillary Tube or Syphon, or only to drop, as if it had not been electrified at all: And likewise, that the Water may be made to run from the same Syphon in a constant Stream, without being made electrical, but cease to run, and only drop, the Moment it becomes electrical. Under the one or other of these Cases, I shall have an Opportunity of taking notice of the several Varieties observable in these Experiments; all of which I shall endeavour to account for from the following general Principles.

First, That the several electrical *Phænomena* are produced by means of *Effluvia*.

Secondly, That the Particles composing these *Effluvia* strongly repel each other.

Thirdly, That the said Particles are strongly attracted by most if not all other Bodies whatsoever.

That the electrical *Phænomena* are produced by means of *Effluvia*, is in general acknowledged by all the Authors who have wrote upon Electricity, however they may differ in Opinion with regard to the Bodies in which they are contained. The Properties I have mention'd of these *Effluvia* may be easily deduced from most of the Treatises lately published on this Subject. But to leave no Room for any Objection, I would beg Leave to observe, that the Existence of these *Effluvia* is proved by all those Experiments in which a Stream of Light is seen to issue from the electrified Body; particularly those Streams which are seen to issue in diverging Rays from the End of the original Conductor, when made of Metal, and reduced to a Point; from their

their being felt to strike against the Hand like a Blast of Wind, when it is brought near the Stream, and from that offensive Smell which generally accompanies these Experiments, and which is always more perceptible, the more strongly the Sphere is excited.

That the Particles composing these *Effluvia* repel each other, appears from those Experiments, in which two Bodies, how different soever they may be in kind, repel each other when they are sufficiently impregnated with these *Effluvia*. As a Feather, by the excited Tube; the several Fibres of the same Feather, or two Cork Balls, which will be found strongly to repel each other, so long as they retain any considerable Quantity of these *Effluvia*. Which Property will always decrease, as the Quantity they contain diminishes.

That these *Effluvia* are strongly attracted by most if not all other Bodies, is so evident from almost all the electrical Experiments, as to make any particular Examples of it needless here; especially as I shall have Occasion to take notice of the strong Attraction between the electrical *Effluvia* and Water, in accounting for these Experiments. And the first, I would take notice of, I shall now proceed to state as follows.

EXPERIMENT I.

If a Vessel of Water is hung to the prime Conductor, having a Syphon in it of so small a Bore that the Water will be discharged from it only in Drops, on the Water's becoming electrical by means of the Machine, it will immediately run in a Stream,

and continue to do so, till the Water is all discharged, provided the Sphere is continued in Motion.

That Water does not run in a constant Stream, but only in Drops, from a Syphon of a small Bore, is doubtless owing to the same Cause by which it is sustained above the Level in capillary Tubes. If therefore Water is made to run in a Stream barely by its being impregnated with the electrical *Effluvia*, it should follow, that if one or more capillary Tubes be placed in a Vessel of Water, that which is sustained in them would either sink down to a Level with the rest of the Water, on its being made electrical, or at least that it would not continue at the same Height as before; but if the Experiment is made, the Water will be found to continue exactly at the same Height, whether it is electrified or not.

Again, if the bare electrifying the Water was the Cause of its running in a Stream, it would continue to run in the same manner, so long as the Water continued electrical, which it will not do: For, on stopping the Motion of the Machine, the Stream will immediately cease, and the Water will only drop from the Syphon, notwithstanding its being strongly impregnated with the electrical *Effluvia*. To account then for the Water's being made to run in a Stream in this Experiment, I would observe, that so long as the Machine is in Motion, there is a constant Succession of the electric *Effluvia* excited, and which visibly run off from the End of the prime Conductor in a Stream, and as they are in like manner carried off from all Bodies hung to it, those *Effluvia* which run off from the End of the Syphon, being strongly attracted by the Water,
carry

carry so much of it along with them, as to make it run in a constant Stream.

That the Attraction between the Water and electric *Effluvia* is sufficient to produce this Effect, might be proved by a Variety of Experiments; but I shall only observe, that to this Attraction it is owing that silk Lines and glass Tubes (which, from their imbibing so very small a Quantity of these *Effluvia*, are generally made use of as Supports in many of the electrical Experiments) on only being wetted become strong Conductors: And that if an excited Tube is held over a Vessel of Water, the Water is found to imbibe a very considerable Quantity of this electric Matter; and, on the Approach of a Finger, or any other non-electric Body, the Water will be perceived to rise towards it; and if the Finger is brought so near the Surface as to draw off the *Effluvia*, they will carry several Particles of the Water along with them towards the Finger, in a Direction directly contrary to that of Gravity; and therefore may well be supposed, when acting in the same Direction, to have an Influence sufficient to produce a Stream, as in the Experiment.

And that this Current of the electric *Effluvia* is the true Cause why the Water runs in a Stream from the End of the Syphon, is farther evident, in that whatever tends to increase or diminish the Current of the *Effluvia*, produces the same Effect upon the Water. I have already observed, that when the *Effluvia* are strongly excited, they will be seen to pass off from the End of the prime Conductor in luminous Rays; and the same may be observed with respect to those which pass with the

Water from the End of the Syphon; but if any non-electric Body is brought under the Syphon, as, by its Attraction, the Current of the *Effluvia* will be increased, so these luminous Rays will likewise extend to a greater Length. Again, if the Motion of the Machine is stopped, the Current of the electric *Effluvia* will thereby be stopped, and the Water will immediately cease to run in a Stream, notwithstanding its being strongly impregnated with the electrical *Effluvia*.

And that the Water is strongly impregnated will not only appear from the Drops being sooner divided into small Particles than they would be if they had not been electrified, but from those Particles being separated to a greater Distance from each other, by the repulsive Property of the electric *Effluvia*; and if any of the Water is received into a dry glass Vessel, on the Approach of a Finger towards its Surface, there will be seen a Spark to issue from it in the same manner as from Water electrified by an excited Tube; or if any non-electrical Body is brought under the Syphon, by whose Attraction the *Effluvia* may be drawn off, the Water will immediately be found to accompany it in a Stream.

EXP. II.

If the Vessel of Water with the Syphon in it is suspended by any non-electric Body over another strongly electrified, the Water will immediately run from the Syphon in a Stream; but if supported by a Piece of Silk, or any other electrical Body, the Water will immediately cease running, and only be
discharged

discharged in Drops. These *Phænomena* may, from what has been already said under the former Experiment, be easily accounted for.

That the Water is made to run in a Stream, is plainly owing to the mutual Attraction between the electrified Body and the Water; which Attraction will continue, so long as the Vessel which contains the Water, by being supported by a Non-electric, is prevented from retaining any of the electrical *Effluvia*; these *Effluvia* being drawn off by the non-electric Body, to which the Vessel is suspended: But on the contrary, when the Vessel is suspended by an Original-Electric, the *Effluvia*, not being attracted thereby, will be prevented from running off, and the Water will soon be found to have imbibed a Quantity of them, sufficient, by their repelling Property, to greatly weaken, or wholly to destroy, the former Attraction, when the Water will cease to run in a Stream, and only drop, as if it had not been held near any electrified Body. Mons. *L'Abbe Nolet* has endeavoured to account for the former Part of this Experiment, by supposing there is, what he calls, both an affluent and an effluent electric Matter; but he takes no notice of the latter Part, which is not easily solved upon his Supposition. But if what I have observed on these Experiments is satisfactory, I apprehend I have accounted for the several *Phænomena* on much more solid Principles, and that thereby any less certain Hypothesis is render'd useless.

I intended to have taken some notice of the different Acceleration of the Fluids thro' Tubes of different Bores; but as this Acceleration will always
vary

vary with the Current of the electrical *Effluvia*, unless some Method could be found out to render this Current uniform throughout the whole Series of Experiments, the Prosecution of this Inquiry will be rendered extremely difficult, and the Result will at best be very uncertain. I am Sir, with the greatest Respect,

Your most obedient humble Servant,

John Ellicott.

When the foregoing curious Letter was read at the Meeting of the *Royal Society* on *Thursday* the 25th of *February* last, 1747. I acquainted the Gentlemen present, that the same ingenious Author had communicated to me a Paper several Months before, in which he had more fully and particularly delivered his Thoughts on the surprizing *Phænomena* of Electricity, and as several Persons expressed their Desire of seeing that Paper, I requested of him either a Copy, or an Abstract of the same; in Compliance with which he, some Days after, gave me the two following Papers, containing the Substance of what he had before shewn me; and I immediately put them into the Hands of Dr. *Mortimer*, one of the Secretaries of the *Society*, who read them at the two Meetings of the *Society*, on the several Days noted at the Head of those Papers.

M. Folkes.

2. *An Essay towards discovering the Laws of Electricity, addressed to the Royal Society.*

Read March 24.
1747-8.

THE great Difference I observed in the Sentiments of those ingenious Gentlemen who have favoured us with their Discoveries in Electricity, made me very desirous of finding out some general Principles, by means of which I might be able to form a Judgment of the several Hypotheses whereby they have endeavoured to account for the principal *Phænomena* observable in those Experiments. In order to this I took a general Survey of all the more remarkable Experiments, and out of them made Choice of such as I judged were most proper for my Purpose; and from these I deduced the general Principles hereafter mentioned. The Advantage I promised myself from this Method was, that the plainer and more simple the Experiments were, which I made choice of, the less liable I should be to mistake in any Conclusions drawn from them; and that every fresh Experiment, I could account for by them, would be an additional Proof in their Favour; and if my Attempt in explaining the following Experiments from those Principles should prove satisfactory, the Truth of them would be thereby so fully confirmed, that we might safely rely on them in forming a Judgment of any of the Discoveries already made; and (how general soever they may seem to be) I doubt not but they will be found of Service in prosecuting our future Inquiries on this Subject.

The Experiments from which I deduced these Principles were these which follow.

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EXPERIMENT I.

If a glass Tube is rubbed by a very dry Hand, and a Finger is brought near any Part of it, a Spark of Fire will seem to issue from it, and strike against the Finger; and if the Finger is carried at a like Distance from the End of the Tube towards the Hand in which it is held, a Number of Sparks at a small Distance from each other will be seen coming from it, and a snapping Noise will be heard. The Tube is then said to be excited, or to be electrical; and at some times, when it is strongly excited, Sparks will issue from the Tube in Streams, not only while it is rubbing, but will continue to dart out from it for a considerable time after the Rubbing has ceased, and a very strong offensive Smell will be perceived.

EXP. II.

If the Tube, when thus excited, is held over some Pieces of Leaf-Gold, or any light Bodies whatsoever, they will be attracted towards it; and the more strongly the Tube is excited, the greater Distance they will be attracted from; and when they come near the Tube (tho' without touching it) they will be repelled from it, and continue to be so, unless touched by some other Body, when they will be attracted by the Tube as before: But if the Tube is but weakly excited, they will be attracted quite to the Tube, to which they will sometimes adhere, without being repell'd from it.

EXP.

EXP. III.

If a Ball (of Cork suppose for Lightness) be hung by a silk Line, and the excited Tube is applied to it, it will not only be attracted, but will have an attractive Quality communicated to it from the Tube; and if any light Bodies are brought near the Ball, they will be attracted by it.

EXP. IV.

As the Tube, when strongly excited, will not only attract, but afterwards repel any light Bodies brought near it, in like manner the Cork-Ball will be endued with the same Property; so that a smaller Ball will first be attracted towards it, and then repelled from it, the same as the Leaf Gold in *Exp.* 2. and on touching any other Body it will be again attracted; and this may be repeated several times, provided the smaller Ball is much less than the larger one, tho' the Effect will constantly grow weaker and weaker, as every time the lesser Ball is attracted, it carries off with it some of the electric-Virtue, and is likewise endued with the same Properties as the larger Ball.

Mr. *Gray*, Mr. *Dufay*, and others have observed, that this electrical Quality is not only to be excited in Glass, but in most solid Bodies capable of Friction (Metals excepted); tho' in some it will be scarcely sensible, and that it is found to be strongest in Wax, Resins, Gums, and Glass: And as Glass is the easiest procured of a proper Form, it has generally been used in making these Experiments. It has been fur-

ther observed, that those Bodies in which the electrical Quality is capable of being excited the strongest by Friction will receive the least Quantity of it from any other excited Body, and therefore are properly made use of to support any Body design'd to receive the electrical Virtue. The Truth of this will sufficiently appear from the following Experiment.

Exp. V.

Hang up two Lines, one of Silk, and the other of Thread; that of Thread will be attracted by the Tube at a much greater Distance than the Silk. Again; Fasten to each String a Feather, or other light Body; if the Tube is brought to the Feather fastened to the Silk, it will be first attracted, and afterwards repelled; and from the Virtue communicated to it from the Tube, the several Fibres of the Feather will strongly repel each other. But when the Tube is brought to the Feather fastened to the Thread, the Feather will be strongly attracted, and continue to be so, without ever being repell'd, the Virtue passing off by the Thread it is hung to. If a glass Ball is hung to the silk Line, it will be but weakly attracted by the Tube; but one of Cork or Metal much stronger.

Exp. VI.

Let a Rod of Iron be sustained by silk Lines, and by means of a glass Sphere (which can be more regularly and constantly excited than a Tube) be made electrical; it will be found to have all the Properties of the excited Tube mention'd in *Exp. 1.* A
Stream

Stream of Light will come from the End of it, if it is pointed; it will attract, repel, and communicate this Virtue to any other non electric Body: On the Approach of a Non-electric, a Spark of Fire, with a Snap attending it, will come from it; which Spark will be greater or less, as the Bodies approaching it have more or less of the electrical Quality residing in them; and there will likewise be the same offensive Smell as was observed of the Tube.

From these Experiments, which I think contain the principal *Phænomena* of Electricity, may justly be drawn the following Conclusions:

- 1st. That these remarkable *Phænomena* are produced by means of *Effluvia*; which, in exciting the electrical Body, are put into Motion, and separated from it.
- 2^{dly}. That the Particles composing these *Effluvia* strongly repel each other.
- 3^{dly}. That there is a mutual Attraction between these Particles, and all other Bodies whatsoever.

That there are *Effluvia* emitted from the Tube when rubbed, and which surround it as an Atmosphere, is evident, from that offensive Smell arising from them, from that Sensation on the Hands or Face, when the Tube is brought near either of them, and from those Sparks of Light, on a still nearer Approach of the Finger to it.

That the Particles of these *Effluvia* repel each other, is proved by the Cork-Balls (*Exp. 4.*) and the Fibres of the Feather (*Exp. 5.*) repelling each other, when impregnated with them; and by the Leaf-Gold (in *Exp. 2.*) being repelled by the Tube, and
not

not returning to it again, until, by coming near, or touching, some non-electric Body, the *Effluvia* are drawn off from it.

From this Property it is, that these *Effluvia* expand themselves with so great a Velocity whenever they are separated from the electric Body; and as they are likewise capable of being greatly condensed, may we not from hence justly conclude they are elastic?

That there is a mutual Attraction between these *Effluvia* and most other Bodies, appears from their collecting from the Tube such Quantities thereof, as to endue them with the same Properties with the Tube itself, as was proved by the 3^d, 4th, and 5th but more particularly by the 6th Experiment.

These Principles being admitted, it will follow, that the greater Difference there is in the Quantity of electrical *Effluvia* in any two Bodies, the stronger will be their Attraction. For, if the *Effluvia* in each are equal, instead of attracting, they will repel each other; and in proportion as the Quantity of electric Matter is drawn from one of the Bodies, will the Attraction between them increase, and consequently be strongest, when any one of them has all the electrical Matter drawn from it.

The Particles of these *Effluvia* are so exceeding small, as easily to pervade the Pores of Glass, as is evident, in that a Feather, or any light Bodies inclosed in a glass Ball hermetically sealed, will be put in Motion on the excited Tube being brought near the Outside of it; and it has been generally thought that they pass through the Pores of the densest Bodies; and there are several Experiments which

which render this Supposition not improbable; tho' I must acknowledge I have not yet met with any one that I think is quite conclusive.

I shall now proceed to shew, how, from these Principles, the *Phænomena* of some of the more remarkable Experiments of Electricity may be accounted for.

EXP. VII.

Let a Rod of Iron, pointed at one End, be suspended on silk Lines, as in *Exp.* the 6th, and by the Sphere be made electrical. When the Rod is strongly electrified, a Stream of Light in diverging Rays will be seen to issue from its Point; and if any non-electric Body is held a few Inches from the Point, the Light will become visible to a greater Distance, and if the non-electric Body is likewise pointed, a Light will seem to issue from that in diverging Rays in the same manner as from the electrified Rod. But if the non electrical Body is flat; and held at the same Distance from the Rod as the pointed one was, no Light will be seen to come from it.

The principal *Phænomena* to be accounted for in this Experiment are; Why a Light is only seen at the Point of the Rod, and not through the whole Length of it? Why this Light is visible to a greater Length, when the Point is approached by a Non-electric? And, Why a Light is seen to issue from the Non-electric when it is pointed, and not when it is flat.

Upon which I observe, that whenever the Sphere is excited, the electrical *Effluvia* are thereby put into Motion, and made to form an Atmosphere

round about it, from whence, by their repulsive Property, they endeavour to expand themselves on all Sides equally; but being strongly attracted by the Iron, a great Part of them are drawn off along the Rod, about whose Surface they likewise form an Atmosphere, which will be denser or rarer, in proportion as the Attraction of the Rod is greater or less; and as the repulsive Power of these *Effluvia* will always increase in proportion with their Density, it will follow, that whenever the Sphere is so strongly excited, that the *Effluvia* surrounding it are denser than those surrounding the Rod, they will, by their repulsive Property, drive the *Effluvia* off from the End of it in a Stream, and that with a very great Velocity; as is evident, from their striking against the Hand like a Blast of Wind when brought near the End of the Rod: And as this Velocity is partly owing to the Attraction of the Rod, so this Attraction continuing quite to the End of it, the Velocity of the Particles will there be greatest; and as they approach towards the Point, they will be brought nearer together, and therefore become denser there than in any other Part of the Rod; and therefore if the Light is owing to the Density and Velocity of the *Effluvia*, it will be visible at the Point, and no-where else.

And that the Light is thus produced, will appear, in that whatever increases or diminishes either the Velocity or Density of the Particles will increase or diminish the Light. For, let the Motion of the Wheel which turns the Sphere be stopped, the Current of the *Effluvia* will likewise be stopped, and the Rays of Light will no longer be seen to issue from the Point,

Point, and yet the whole Rod will continue to be electrical ; but, on putting the Sphere again into Motion, the *Effluvia* will become visible as before, and will increase, as the Sphere is more strongly excited. Again, the Light will be visible to a greater or less Distance, as the Point is more or less acute ; and as this Light is always brightest next the Point, and grows fainter, as the Rays diverge, this is plainly owing to the different Density of the Rays at equal Distances ; for, when the Point is more acute, the Rays will diverge less, and therefore will be denser to a greater Distance than when it is less acute.

When a Non electric, whose End is flat, is brought within a few Inches of the Point of the electrified Rod, the electric Stream will be attracted by it, and the Rays made to diverge less than before ; and the Effect will be the same as if the Point was more acute ; *viz.* a Continuation of the Light to a greater Distance, and which will be farther increased by the additional Velocity the Particles will acquire from the Attraction of the Non-electric. What will follow on a nearer Approach of the Non-electric to the Rod, will be consider'd under the next Experiment.

If the Non-electric is pointed, and held in the same Place as the former, a Light will appear from it the same as from the electrical Body : For, as the Points of the two Rods are the Parts which approach nearest each other, the Attraction there will be strongest : The Rays therefore, which diverged from the electrical Rod, will be attracted by, and made to converge towards, the Point of the non-electrical Rod, and will consequently be nearly of the same Density at the one as the other ; and the

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Velocity being accelerated by the additional Attraction, the Rays will become luminous at the Point of the Non-electric, the same as at the Point of the electrified Rod. If this Experiment is made with a Tube, instead of a Sphere, as it cannot be so uniformly excited as the Sphere, the Light will issue from the Rod in Flashes, as the Tube is more or less excited.

Several very ingenious Gentlemen, and in particular the Abbé *Nollet* have imagined, that the Light seen at the Point of the Non-electric was produced by means of *Effluvia* issuing from it in diverging Rays towards the electrified Rod, and which Current of *Effluvia* is therefore supposed to be the Cause of the attractive, as a like Current issuing at the same time from the electrified Rod is supposed to be the Cause of the repulsive Property of Electricity.

This Conjecture being directly contrary to the Account I have given of this *Phænomenon*, I shall offer some Considerations in Support of what I have advanced, and which I think will make it appear highly improbable, that any such Current of *Effluvia* issues out of the Non-electric; but as what I have to offer on this Subject would trespass too much on the *Society's* Time at present, I shall defer it to my next Paper. I am,

Gentlemen,

Your obliged humble Servant,

John Ellicott.

3. *A Continuation of the foregoing Essay.*

Read May 19
1748.

IN a Paper I had the Honour to communicate to this *Society*, *March 24*, I endeavour'd, from the Principles therein laid down, to account for some of the most remarkable *Phænomena* of Electricity; and in particular for that Appearance of a Light issuing from the End of an iron Rod, when pointed, and made electrical; Why this Light was visible only at the Point, and in no other Part of the Rod: Why the Light was visible to a greater Length when the Point was approached by a Non electric: And why a Light will be seen as issuing from the Non-electric when it is pointed, but not when it is flat.

I shall now endeavour, from the same Principles, to account for those *Phænomena*, which will be produced on a nearer Approach of the Non-electric to the electrified Rod.

EXP. VIII.

If the non-electric Body, whether flat or pointed, is brought nearer to the End of the Rod, than in the last Experiment, there will be a small Stream of Light produced, reaching quite from the electric to the non-electric Body; and if brought still nearer, there will issue a Spark attended with a small snapping Noise, which will be succeeded by others at equal Intervals; and if the Non-electric is held at some Distance from the Side of the Rod, the Point of it will frequently appear luminous, but no Part

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of the electrified Rod will be so. If it is brought nearer, there will likewise be Sparks produced at nearly equal Intervals from each other, which will sometimes appear as issuing from the Side of the electrified Rod, at others, as coming from the Non-electric.

If a Finger is used as the Non-electric, it will receive a smart Stroke; and if Spirit of Wine, heated so as to emit an inflammable Vapour, is made use of, it will be kindled by the Spark.

These *Phænomena* may, on the afore-mention'd Principles, be thus accounted for.

If the non-electric Rod is pointed, and brought so near, as, by its Attraction, to prevent the Rays issuing from the Point of the electrified Rod from diverging, they will be drawn off parallel to each other, and consequently be equally luminous throughout the whole Distance between the two Rods.

If the Non-electric be brought still nearer, the attractive Force will be so much increased, as not only to affect the *Effluvia*, when they are driven off from the Point of the electrified Rod, but to be capable of drawing them off from a considerable Part of the Rod beyond the Point; and that with a Velocity, and in a Quantity, sufficient to occasion both the Spark and Blow, as well as the Noise that is heard.

The same is the Case, when the non-electric Rod, or a Finger, is held against the Side of that which is made electrical: At a greater Distance a Light will appear as issuing from the Non-electric, the Particles attracted from a large Surface of the Rod (and therefore not visible as coming from it) being made to converge to a Point, are thereby rendered

dered luminous, and, if brought nearer, there will issue Sparks in the same manner as when held to the End: And that this is owing to the Increase of the attractive Force, seems plain; for it was observed in the last Experiment, the Attraction was capable of changing the Direction of the Rays at the Distance of several Inches; whereas a Snap or Spark is seldom produced, when the Non-electric is held more than an Inch and half distant. If therefore the Attraction decreases, as the Squares of the Distances increases, as it probably does, the attractive Force will be many times greater in one Case than in the other, and if where the attractive Power was weaker, as in the former Experiment, there were so many Rays of the electric Matter collected, as to be sufficient to produce a Light, it cannot be thought extraordinary, when the Attraction is so greatly increased on the nearer Approach of the Non-electric, that both the Density and Velocity of the Particles should be thereby increased, so as to produce Heat sufficient to fire the Vapour arising from Spirit of Wine, or any other inflammable Vapour.

And that the Quantity of the electric Particles is greatly increased, as well as their Velocity, is evident from that large Surface of the Rod, which, by the Approach of a Finger, is in one Spark divested of them; and which requiring some time before it can be again sufficiently recruited, I apprehend is the Reason of that Interval between the Sparks. And here it must be observed, that the Distance the Point of the Non-electric is held at from the Rod, in order to produce the greatest Spark, must be varied, in proportion as the Rod is electrified in a greater or less Degree; the more strongly the Rod

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is impregnated the greater will be the Distance; and if then the Non-electric is brought nearer, the Sparks will be smaller, but succeed each other quicker; so that when it is brought almost to touch the Rod, they will appear like a small Stream. The Reason of which I take to be, that as the electric Atmosphere surrounding the Rod is denser nearer it than farther off, when the Non-electric is brought into so very dense a Part of the Atmosphere, it will from thence become nearly as electrical as the Rod itself; and therefore lose great Part of its attractive Force, and consequently will only be able to draw off those Particles from the Rod which are nearest to it.

I would farther take notice, that the Sparks are always produced in the Space between the Non-electric and the Rod, and often appear as issuing from the Non-electric. This Appearance is probably owing to those Particles, which, by their Elasticity, are reflected back again from the Non-electric towards the Rod; and which, by striking against those coming from it, produce both the Sparks and Noise that is heard; and as I have already shewn, that the Particles often appear in luminous Rays at the Point of the Non-electric, it thence happens, that the Spark is frequently kindled so near to the Non-electric, as to appear as issuing from it.

I observed, in my former Paper, that several ingenious Gentlemen, from this Appearance of a Light at the Point of the Non-electric, have imagined there was a Current of electrical *Effluvia* continually issuing out of it, and which, setting in towards the electrified Rod, was the Cause of the Attraction of the Electricity: And this Conjecture of theirs will

will seem to be greatly favoured by the following Experiment.

If some of the Fibres of a Down-Feather be fastened to the End of a small Skewer or Wire, and made electrical; they will strongly repel each other, and will expand themselves on all Sides to the greatest Distance possible from each other; but if a non electric Person bring the Point of a Pair of Compasses, or any other small-pointed Body near them, they will be repell'd from it, and driven up together as with a Blast of Wind, and, in the dark, a Light will be seen as issuing from the Point; from whence it might be concluded, that the Fibres are repell'd by *Effluvia* issuing out of the Point of the Non-electric.

As the Abbé *Nollet* endeavours to account for the Attraction of Electricity on this Principle, I shall offer some Considerations, which, notwithstanding these Appearances, have induced me to be of a different Opinion; and they are founded on the following Observations.

First, That however replete any Bodies may be with the electric Matter, none of these *Phænomena* are ever produced, unless the *Effluvia* are first excited in some particular Body, and put in Motion, either by rubbing, or some such-like Operation.

Secondly, That the *Effluvia* are not to be equally excited in all Bodies, but much stronger in some than in others; and that, in particular, they are not capable of being at all excited in Metals by Friction.

Thirdly,

Thirdly, The attractive and repulsive Property will be stronger or weaker in any Body, in proportion to the Quantity of excited *Effluvia* wherewith it is impregnated.

Fourthly, That those Bodies which are most easily excited by Friction, will receive the least Quantity of the electrical *Effluvia* from any other excited Body; and, on the contrary, Metals, or those Bodies in which they cannot be excited by Friction, will receive the most.

From these Observations I think it may be shewn, that this Appearance of Light is so far from proving that the *Effluvia* come out of the Non-electric, at whose Point they are visible; that from thence it cannot be concluded the Body has any of the electrical Matter residing in it, but is rather a Proof to the contrary. For I have already shewn, that the same Appearance would be produced from the setting in of the *Effluvia* into the Non-electric; and this might be confirmed, if necessary, by a Variety of Experiments. And as those Bodies, at whose Point this Light appears the strongest, afford us no Signs of their having any of the electrical *Effluvia* residing in them, either by their attracting or repelling other Bodies, or by their being capable of being excited in them by Friction, as in Glafs, &c. nor in short any sort of Evidence whatsoever, but what arises from this Appearance; may we not expect some better Proof of their being possessed of these *Effluvia*, before we admit of their issuing out of them?

Again, it appears very extraordinary, that those Bodies, in which the *Effluvia* cannot be excited by any

any other Method, should send forth such Streams of them, only on their being brought within a few Inches of the electrified Rod, and that these Streams should increase as the Rod is more strongly electrified ; and yet that few or none of these Streams should issue from those Bodies in which the *Effluvia* can be excited : And if the first-mention'd Bodies are themselves strongly impregnated, the Streams will disappear, and they will be so far from parting with any of their *Effluvia*, that, on the contrary, they will be strongly repelled by the Rod.

I farther apprehend, on this Supposition, it will be extremely difficult, if not impossible, to account for the ceasing of the Stream from the Point of the Non-electric on stopping the Machine ; as likewise that the Rod should so soon be divested of its *Effluvia*, on such a Non-electric's being held near it, which it would otherwise retain for several Hours, and which I think is a strong Proof of the *Effluvia's* passing from the Rod into the Non electric. And that it certainly does so, may be confirmed by the Person who holds the Non-electric stepping upon a Cake of Wax, when he will soon become electrical, from the *Effluvia* he will receive (thro' the Point of the Non-electric) from the Rod ; but so long as he continues to se so, there will not be seen any Light to issue from the Point ; which I apprehend cannot be accounted for on any other Principle, but that of the setting in of the *Effluvia* at the Point of the Non-electric. And as I have already shewn, that all the *Phænomena* are naturally to be accounted for on this Principle, without being liable to any of the above-mention'd Objections, I must remain of the Opinion (till I can see

these Objections answer'd) that this Appearance of Light is no Proof that the *Effluvia* issue out of the Non-electric, but of the direct contrary.

The above-mention'd Objections might be brought, with equal Force, against the Fibres of the Feather being repelled by *Effluvia* issuing out of the Point of the Non-electric that is held near it, and in particular, that this Effect would cease to be produced, either when the Machine was stopped, or the Person who held the Point became electrical. And to these I would add, that if this was really the Case, the Fibres would continue to be repell'd, notwithstanding any Alteration in the Shape of the Non-electric; whereas, on the contrary, if the Joint of a Pair of Compasses was held towards them, instead of the Point, they would be strongly attracted to it: And the same will always happen, whenever an obtuse Body is brought near them instead of a pointed one.

The true Cause of this remarkable *Phænomenon* I apprehend to be the different Density of the *Effluvia* at the Extremities of the two Bodies; for I have already shewn the *Effluvia* will be much denser at the Extremity of a pointed Body than at an obtuse one: And as the Force by which the Particles endeavour to expand themselves, increases in proportion to their Density, it follows, that the Particles will be reflected back with greater Violence from the pointed Body than the other; and this Force exceeding the attractive Power of that particular Part of the Feather, to which it is directed, the Fibres will be repelled by it; whereas the Force, with which the Particles endeavour to expand themselves from the obtuse Body, being less than the attractive Power,
it

it follows, that the Fibres of the Feather will continue to be attracted by it.

EXP. IX.

Take two Plates of Metal, very clean and dry, whose Surfaces are nearly equal; hang one of them horizontally to the electrified Rod, and bring under it upon the other any thin light Body, as Leaf-Silver, &c. when the upper Plate is made electrical, the Silver will be attracted by it; and if the under Plate is held at a proper Distance, will be perfectly suspended at right Angles to the Plates, without touching either of them; but if they are either brought nearer together, or carried farther asunder, the Leaf-Silver will cease to be suspended, and will jump up and down between them. The same Effect will be produced, if you reverse the Experiment, by electrifying the bottom Plate, and suspending the other over it.

If the upper Plate is electrified when the Leaf-Silver is brought near, it will be attracted upwards by it, and thereby become electrical; and so long as it continues to be electrical, it will likewise be attracted downwards by the non-electrical Plate. Whenever therefore this last Attraction added to the Gravity of the Silver, which acts in the same Direction, is equal to the contrary Attraction upwards, the Leaf-Silver will, by means of these two opposite Forces, be kept suspended between the Plates, and will continue to be so, as long as the Equality of these Forces is preserved.

Ff 2

I have

I have already shewn, that the Attraction between any two Bodies will always be in proportion to the different Quantity of electric *Effluvia* they are possessed of; the greater that Difference is, the greater will be the Attraction. In order therefore to obtain this equal Attraction at first, the Leaf-Silver must be imbued with a greater or lesser Quantity, in proportion as the Plate is more strongly or weakly electrified; but always with a much less Quantity than the Plate; and likewise the lower Plate will require to be placed at different Distances, in proportion to the Quantity of electric Matter the upper Plate is possessed of. As therefore the Suspension of the Silver depends upon the exact Proportion of Attraction (arising from the different Quantities of electric Matter) in the two Plates and Leaf-Silver, it follows, that whatever alters the Quantity contained in any one of them would prevent the Suspension.

It is well known, that, by the Attraction between any two Bodies, the electric *Effluvia* are continually drawn off from that which has the greatest Quantity of them, till the other being sufficiently impregnated, the Attraction ceases. In order therefore to preserve these Proportions, it is necessary, that, as fast as the non-electric Plate draws off any of the *Effluvia* from the Leaf-Silver, it should part with it again; and so, by continuing to be a Non-electric, an equal Degree of Attraction be preserved; and again, that the Leaf-Silver should receive a fresh Supply from the electrical Plate, equal to what it constantly parts with; and the electrical Plate must likewise receive an equal Supply from the Globe; and

and that there is such a constant Current of the electrical *Effluvia*, is evident, from those small Streams of Light, visible at the two Corners of the Silver next the Plates. If therefore the Globe should be stopped, or the under Plate by any means become electrical, these Proportions would be thereby destroy'd, and the Leaf-Silver would cease to be suspended.

That the Leaf-Silver is always nearer to the non-electrical than to the electrified Plate, is owing to its receiving its Supply of *Effluvia* from the Atmosphere surrounding the electrified Plate: For as the Plate is more strongly electrified than the Silver, its Atmosphere of *Effluvia* will be denser to a greater Distance than that surrounding the Leaf-Silver, and therefore can supply an equal Quantity at a greater Distance than what the lower Plate can receive from the Silver, whose Atmosphere is rarer; and therefore, as the Silver will always be suspended in that Part where the two Currents are equal, without which I have already shewn the Proportion would be destroy'd, it will consequently be always nearer to the non-electrical than to the electrified Plate. If the Experiment is reversed, by electrifying the under Plate, and making the upper one the Non-electric, the only Difference will be, that the Gravity of the Silver must then be added to the Attraction of the electrified Plate, and will therefore cause the Silver either to be nearer the non-electrical one, or the Plates to be moved a little farther asunder, or perhaps both.

I shall not at present presume to take up any more of the Time of this illustrious *Society*; hoping that

that I have already shewn how the principal *Phænomena* of Electricity may be accounted for, upon the few Principles I have laid down; and however in different Experiments the Effects produced may either be varied, or increased, I doubt not but they may all be easily accounted for from the same Principles; as I shall willingly attempt to make appear at some more convenient Time, should it be thought necessary. In the mean time I have the Pleasure to subscribe myself,

Gentlemen,

Your most obedient humble Servant,

John Ellicott.

XII. *A brief account of a Roman Tessera, by Mr. John Ward F. R. S. & Prof. Rhetor. Gresh.*

Read March 3. 1747. **T**HE brass plate, which accompanies this paper, and has been the occasion of it, was dug up some time since at Market-street in Bedfordshire; which lies in the Roman road called *Watling Street*, about five miles on this side Dunstable; and was brought to the *Society* by their worthy Member, *Samuel Clark Esq.*

The

The inscription engraven on the two sides is,

TES. DEI. MAR
SEDIARVM

See the
TAB.

Which Words may, as I apprehend, be read at length in the following manner :

Tessera Dei Martis Sedarum.

The first abbreviated word TES. I take to stand for *Tessera*, a *dye* or *cube* (a), so called from the Greek word τέσσερα or τέσσερα, *four*; respect being had to its number of sides, distinct from the two horizontal planes, above and below. And under this consideration it was distinguished from the *Talus*, which being round at each end contained only four planes or faces, whereon it could stand, and therefore when thrown had no more than two side faces in view. Hence *ludere talis et tesseris* are spoken of by Roman writers as two different games (b).

But if this was the first and original notion of the word *Tessera*, it was applied afterwards to many other things; and that not so much from a similitude in the figure, as from the relation they bore to some other thing, of which they were the sign or token; as the points, on the upper plane of the dye denoted the good or ill success of the cast. To recite the several uses of this word would both be tedious and unnecessary; and therefore I shall mention some few only, from which the design of this plate may the more easily appear.

And

(a) *Macrob. In somn. Scip. lib. 11. cap. 2.*
Jeneſt. cap. 16.

(b) *Sic. De*

And I shall begin with the *Tessera hospitalis*, which was either public or private. As to the former, we find among the inscriptions published by *Gruter* instances of two municipal towns, who put themselves under the patronage of a Roman governor. And the reciprocal engagement between them, which was engraved on two copper plates, in the form of an oblong square, with a pediment at the top, is called in both *TESSERA HOSPITALIS* (a). The design of the latter was to cultivate and maintain a lasting friendship between private persons, and their families; and gave a mutual claim to the contracting parties, and their descendants, of a reception and kind treatment at each others houses, as occasion offered. For which end it was requisite, that those *Tesserae* should be so contrived, as might best preserve the memory of that transaction to posterity (b). And one method of doing this was by dividing one of them lengthwise into two equal parts, upon each of which one of the parties wrote his name, and interchanged it with the other. A draught of one made of bone, and so divided, may be seen in *Thomasinus*, with the name of the person on each part. Upon one of them is,

POLYNICES

ASCANIO.F.

And upon the other,

CLAPHYR

ANDRAE.M.I.F.

The

(a) *Pag.* CCCLXII, CCCLXIII.
22. v. 2. 87,

(b) *Plaut, Pœnul.* v. 1.

The names are written on the inside ; and when the two parts were put together, they made a cylinder (a). From this custom came the proverbial expression, *tesseram hospitalem confringere* ; which was applied to those persons, who violated their engagements (b).

The *tesserae frumentariae* are often mentioned by Roman writers, which were small tallics given by the emperors to the populace at Rome, intitling them to the reception of a certain quantity of corn from the public at stated seasons. And those, who were possessed of them, when they did not want the corn, sometimes sold them to others ; as we learn from the satyrist, when he says :

*Summula ne pereat, qua vilis tessera venit
Frumenti* (c).

The person, who had the inspection of these *tesserae*, and distributed the corn to those, who produced them, seems to have been called *tesserarius* ; as *Pignorius* observes from a funeral monument, inscribed SYMPHORO TESSERARIO SER. CAESARIS (d). These *tesserae* were first made of wood, as appears from the words of *Pliny*, where treating upon the nature and properties of trees he says, *Ligustra tesseris utilissima* (e). But *Fabretti* has published the draughts of two of them made of stone, in the form of cylinders, and of the same size with the originals. The length of them is somewhat short of three inches, the diameter three quarters of an inch, and the following

(a) *De tess. hospit. cop.* 15. (b) *Plant. Cistell.* 11. v. 29.
(c) *Juvenal. Sat.* VII. 174. (d) *De servis*, pag. 318. ed.
1674, vet. (e) *H. N. Lib. XVI. cap. 8. § 31.*

lowing inscriptions cut upon them contain the names of the persons, to whom they belonged :

TORQVATVS
CREOP. D. I.

LVPVS
PELORI D. I.

Where D. I. the two last letters in each inscription stand, as he supposes, for *die prima mensis*, the time appointed for receiving the corn (a).

There was also another sort of *tessera*, not much unlike these, which intitled persons to a sight of the public games and other diversions ; but they are generally made in the form of an oblong square. *Pignorius* has given us the draught of one in his own possession, which consisted of ivory. Upon one of the sides is the name PHILOMVSVS PERELI, on the next SPECTAVIT, on the third a *trident*, and upon the fourth a *palm branch* (b) ; the two last of which plainly shew, that it was given for admission to the combats of the gladiators. Others of them had on different sides the name of the person, with the day, on which the show was exhibited, and the names of the consuls at that time. Instances of these may be seen in *Thomassinus*, one of which, as he says, was made of yellow glass (c).

But the *tessera militaris* most frequently occurs in the Roman historians, which was the signal given by the general, or chief commander of an army, as a direction to the soldiers for putting in execution any duty or service required of them. This upon urgent occasions was only vocal ; as for a sudden march,

(a) *Inscript. antiq.* pag. 530.
(c) *De tess. hospital.* cap. 15.

(b) *Ubi supra*, pag. 38.

march, or an attack upon the enemy. But in ordinary cases, as for setting the watch, taking their dinner, or the like, it was written on a tablet. This in either way it was first given by the general to the officers next in rank, and from them to the subalterns, till it came to the person, whose province it was to communicate it to the soldiers in each company. This tablet was commonly made of wood, as appears from *Polybius*, who calls it *ξύληφιον*, a small piece of wood (a). The signal inscribed upon it was very short, and usually comprised in one or two words; as *victoria*, *palma*, *virtus*, *Deus nobiscum*, *triumphus imperatoris*, mention'd by *Vegetius* (b); with many others of the like nature, which may be seen in antient writers. The person, whose office it was to impart the signal immediately to the soldiers, is by *Vegetius* called *tesserarius* (c). Hence in *Gruter's* inscriptions we meet with AVRE. IANVARIVS. TESSERARIVS. LEG. XIII, and C. GALERIO. C. LIB. AGATHON. TESSERARIO. COH. XII. PRAET. MILITVM, as also L. POMPEIO. L. F. POMP. REBVRRIO. TESSERARIO. IN. CENTVRIA... (d). By which different forms of expression compared together one would be lead to conclude, that every century had its *tesserarius*, from whom the soldiers immediately received the signal; and that when the legion or cohort only is mentioned, the meaning is not, that the person named in the inscription performed that office

(a) *Lib. vi. pag. 479. ed. Paris.*

(b) *Lib. iii. cap. 5.*

(c) *Lib. ii. cap. 7.*

(d) *Pag. DCVI. IO. DCVIII. 7. DCIX. IO.*

office to the whole legion or cohort, but only to some particular century in each of them.

But besides these civil and military *tesserae* there were others, which more especially related to religious affairs, and may therefore be called sacred; to which the inscription on this brass plate seems to agree. For the two next words engraven upon it, namely *DEI. MAR.* must, I think, stand for *Dei Martis*. And if the last word *SEDIARVM* be taken for the name of a town, called *Sediae*, this *tessera* may respect the *God Mars*, as the tutelar deity of that place. The religious worship among the Romans consisted chiefly in sacrifices and other public ceremonies, the expense of which in particular places was supported either by the contributions of the inhabitants, or by private gifts. We have an instance of the latter in an inscription first published by *Reinesius*, where it is said, that *L. Veratius Felicissimus*, (a) patron of *Tolentium*, (or *Tollentium* a municipal town in *Italy*) gave to the inhabitants their annual sacrifices, which were offered on the eleventh of May for a plentiful harvest. That inscription is cut on a brass plate in the form of an oblong square, with a female bust in a pediment at the top, designed very probably to represent the deity, to whom they addressed. As the inscription is peculiar in its kind, I shall here give the whole of it, as it stands in *Reinesius*.

TESSERAM.

(a) *Grut. Pag. cxciv. 2.*

TESSERAM. PAGANICAM
 L. VERATIVS. FELICISSI
 MVS. PATRONVS. PAGANIS
 PAGI ♡ TOLENTINES
 HOSTIAS. LVSTR. ET. TESSER.
 AER. EX. VOTO. L. DD
 V. ID. MAIAS. FELICIT. (a).

This is called *tessera paganica*, as I imagine, from its intitling the *pagani*, or inhabitants of that town, to the annual claim of the sacrifices therein mentioned. And so far it agreed with the nature of a public *tessera*, which being lodged in the hands of the proper officer, authorized him to collect the several contributions assigned for such religious purposes. And of this latter sort I take the plate to have been, which makes the subject of our present inquiry; both the form and size of it suiting very well with such a design, as it was portable, and ready to be produced, if occasion required. And agreeably to this notion of the word *tessera* the ancient *Glossaries* interpret *tesserarius* by γραμματεὺς, a *scribe* or *clerk*.

As to the following word *SEDIARVM*, tho it no where else occurs, that I know of; yet this, I presume, can be no just objection against its being taken here for the name of a town, called *Sedbae*; when it is considered, how many instances of the like nature are to be found in the inscriptions collected by *Gruter* and others, which give us the names

names of many antient places in the Roman provinces not mentioned by any other writers. And besides, the form of this word appears analogous to the names of several other Roman towns here in Britain; as, *Durobrouae* Rochester, *Ratae* Leicester, *Rutupiae* Richborough, *Spinae* Spene, and some others. It is not improbable, that this plate was found not far from the place, whose name it bears; and which might be situated among the *Cateuchlani*, as their territories are described by Camden (a). But as I have never before seen, nor heard of any thing similar to it, I would submit what is here offered to the judgement of the curious in these inquiries.

G. C. Feb. 25.

1747.

John Ward.

XIII. *An Account of a very learned Divine, who was born with two Tongues; communicated to the Royal Society by Cromwell Mortimer M. D. & Sec. R. S.*

Read March 10.
1747.

IN a MS. Account of the Life of the Rev. Mr. *Henry Wharton*, Chaplain to Archbishop *Sancroft*, written by himself, I have met with the following Passage:

“ *Mihi quidem ex utero materno exeunti duplex erat Lingua, utraque ejusdem figuræ ac magnitudinis; inferiorem excindendam esse clamaverunt mulieres obstetrices.* ”

(a) *Britann. pag. 275, ed. 1607.*

“obstetrices. Verum id noluit mater puerpera.
 “Pietati ejus obsecundavit fortuna. Lingua enim
 “inferior paulatim emarcuit, et in exiguam piscoque
 “haud majorem lingulam, quæ hodiernum manet,
 “contracta est. Lingua interim superior ad justam
 “crevit magnitudinem, quamplurimis longis pro-
 “fundisque fulcis distincta, an vulneribus laniata,
 “dicam! quæ parallelo situ posita una cum lingua
 “creverunt, neque unquam coitura esse videntur.”

Nat. Nov. ix. 1664. Ob. 1694-5. Mart. 5. Æt. 31.

It appears by this Journal of himself that he was always infirm and sickly. *

XIV. *Upon the Sounds and Hearing of Fishes,*
by Jac. Theod. Klein R. P. Gedan. F.R.S.
or Some Account of a Treatise, intituled, “An
“Inquiry into the Reasons why the Au-
“thor of an Epistle concerning the Hear-
“ing of Fishes endeavours to prove they
“are all mute and deaf;” by Richard
Brocklesby M. D. F. R. S.

Read March 10.
 1747-8.

OUR Author in the first place classifies them into two Orders, the first hath Lungs, the other is furnish'd with Organs analogous to Lungs, which we call Fish-Ears, or Gills: All the Whale-Kind, the Dolphin, Porpoise, and such-like, have Lungs. There are two Families of the second Class, to one of them belongs all that Tribe,

* See the Account of *Margaret Cutting*, who speaks without a Tongue, in these *Transf.* No. 484, p. 621.

Tribe, which have one, two, five, or nine Air-Holes, at the Back, or Sides of the Head, or in their *Thorax*, in which concealed Gills are found: The other Family comprehends all Kinds of Fishes, whose Gills are usually placed on each Side the Back of the Head. Our Author's Antagonist alleges, that all Fishes of both Orders are equally deaf; but that all Naturalists except Mr. *Reaumur* are of a contrary Opinion, that Fishes hear distinctly.

Our Author begins with an Air of Ridicule, and shews how far the Letter-writer is ignorant of the various Opinions, modern as well as antient. Our learned Countryman Mr. *Ray* thinks to reconcile these, by allowing that some hear, while others are deaf; but the greatest Part allow that Fishes actually hear; and most, except *Scheuchzer*, seem agreed about the auditory Passages. But the Letter-writer denies they have any Organs of Voice, merely upon the proverbial Authority, *Mute as a Fish*; hence he concludes they are likewise deaf. But in Answer, 'tis replied, the spouting Whale hath all its internal Organs, precisely similar to the Organs of Voice in ~~other~~ *other* Creatures, and therefore they may answer the same Purposes, may actually serve this End: For when the Whales in the *Greenland* Fishery are struck, they roar frequently so loud, as to be heard at two *French* Miles Distance.

But some of the first Family of our second Class, as the Skate, Lamprey, Conger, and others, our Author hath heard utter some kind of Noise; and gives his Opinion, that most Sorts of cartilaginous Fishes can do the same. From Analogy he argues, that as no Beast, from the Lion to the meanest Animal,

mal, nor from the Eagle to the humming Bird, but can utter a Voice, so he thinks the same general Law is observ'd in the Oeconomy of Fishes: But at the same time our Author here seems to lay too much Weight upon what he supposes final Causes, and metaphysical Arguments, which have in all Ages ruin'd Natural Philosophy.

But the Letter-writer queries, whether Fishes may not be mute in our Air, and yet capable of some Voice in their own Element. Our Author takes the Noise which Carp and such Fish make in hot Weather, on the Surface of the Water, to be a Voice: And this is most remarkable when the Male impregns the Row which the Female has before deposited; yet this is often heard, when the Fish is 6 or 7 Inches under Water. Our Author further enumerates many foreign Fishes, and particularly our Smelt, which put alive into Vinegar hisses very audibly.

The Letter-writer had objected against Fishes, that they have no Occasion for Hearing, because they never copulate, as other Animals do: But our Author describes the Manner of Whales, which is performed as that of other Animals; and observes, that they bring forth their Young alive: These follow the Female, and suck Milk from the Teats, which are placed in them near the Organs of Generation; and in violent Storms the Dam takes her Off-spring into her Mouth, and protects them from Danger. This last is common to several of the Skate-kind.

The Letter-writer alleges, That Fish never sleep; but our Author assures us, all such as have Lungs do in the Night-time, thrusting up their Nostils into

H h

the

the open Air. For others he cannot be positive, as their History is little known.

The Letter-writer premises two Questions; first, Whether Fishes have any Ears? or, If the Gills serve the same Purpose? and answers positively in the Negative to both: And therefore concludes they cannot hear. But our Author asserts, that Snakes, Frogs, Chameleons, and others of the Lizard-kind, actually hear, without any of the usual external *Apparatus* of Hearing. For though they want the Auricles and Ears, yet have they auditory Passages, by which Sound is convey'd, and even internal Organs, to which the *Meatus auditorius* reaches. But our Author farther asserts, that all the Whale-kind, and in general such Fishes as have Lungs, have likewise a *Meatus auditorius*, and the internal Organs of Hearing; and appeals to a public Dissection of a Porpoise, and another Fish of the Whale-kind, made by himself; in which the *Os petrosum*, with the other Parts of these Organs, had been separately shew'd; and calls in the concurrent Testimony of Dr. *Tyson*, in his Anatomy of a Porpoise.

Thus having satisfied us about such Fish as have Lungs, he goes on to consider the cartilaginous Species, such as the Skate, Ray, and kind of Lam-prey, which have Organs of Generation, and copulate like Brutes; yet exclude the *Fetus* while yet in the Egg-State: And this from Analogy, that these, and in general all other Fish, as they have Organs which serve them for Lungs, so they may have what answers in others to the *Apparatus* of Hearing.

In Proof of this he asserts, that all Kinds of Fish but these which have Lungs, are always found to have

have Stones in their Heads naturally form'd, and invariably plac'd in the same Situation, being join'd to the contiguous Parts with Ligaments and Nerves, which take their Rise from the Substance of the Brain; and having examin'd the Head of a Pike minutely with a Microscope, he discover'd the auditory Pores in the Stones, and persuades himself, that three Pair of Stones are to be referr'd to this Use; therefore concludes, as there is some Analogy in the Organs, that all Fishes in some measure hear.

The Letter-writer farther objects, that Water is not the Medium of Sounds; and though Air is actually contain'd in all Water, yet it cannot be put into Undulations, any more than the circumambient Water; but that would require a much greater Vibration than the external Air can give. Thus, says he, if a Person immerge his Head a Foot under Water, he will hear nothing but a boiling Din; and however great a Noise is made in the open Air, the Event will be still the same; and if the Water itself be put into the most violent Agitation, the Person will discover no Odds in that Sensation of his Ears from what he perceived in the stillest Water. Hence he concludes Water incapable of transmitting Sounds. Our Author replies, That as Fishes are unanimously agreed to be capable of smelling, so, by Analogy, it is probable they have Hearing; for Odours are convey'd by the Air, as well as Sound. But he thinks the unnatural Position of a Man's Head immerg'd a Foot under Water may be some Cause for that confused Noise, and opposes the experimental Testimony of Abbé *Nollet* himself, who went different Depths under Water, to satisfy

himself how far Sounds could be convey'd in that Medium.

At four Inches under Water he heard the Sound of a Gun discharged, of a Clock striking, and of a Hunter's Horn : These, repeated at different Depths, were heard first at 4, then at 8, afterwards at 18 Inches, and lastly at two Foot. A Man's Voice was also heard in the same Manner.

At different Altitudes of Water, none of them exceeding two Feet, he could perfectly distinguish mixt Sounds, when two Bells were struck, or two Pipes sounded together.

He could distinguish under Water, very distinctly, Words utter'd aloud : And prov'd this Assertion, by declaring, when he came above Water, what was said while he was under it.

All Sounds were heard more faintly, and attenuated ; yet the Difference of the Sound, at 4 and 18 Inches Depth, was not answerable to the Difference of the Altitude of Water.

He observ'd at first, that momentary Sounds were not so well convey'd as continu'd ; yet he afterwards determin'd, at the same Depth, one Tap of a Drum-head, as plainly as a continued Round. This he thinks was the same in a Man's Voice, and the Sound of a Pipe ; but ingenuously owns, he was not fully satisfied in this Experiment ; and therefore does not lay as great Stress on its Certainty as on the former.

Lastly, he held his Head under the Surface of the Water, so as barely to cover him ; but could not hear the Clock strike, which was audible in the open Air at 45 Feet Distance, especially on a Plain.

The

The Abbè therefore concludes, if Fishes do not actually hear, 'tis for want of proper Organs, and not because the Medium cannot convey Sounds.

Our Author mentions the common Notion of Carp, and other Fish, coming out of their Holes at the Sound of a Bell to be fed; and adds a Story, which Mr. *Boyle* somewhere relates, that near *Geneva* a Man had a Fish-pond, whose Banks were so high from the Plain on which it was, that one could not look over them into the Pond; and therefore it was impossible the Fish could see the Person; yet they were at any time conven'd at certain Sounds by the Gardener, in order to be fed, as a creditable Person asserts.

The Letter-writer, having made a high Partition in a Pond, watch'd while an Accomplice behind it made a very great Noise, and discharg'd a Gun, in order to frighten the Fish (if possible) that were playing on the Surface of the Water; but they did not give any Attention; yet as soon as ever they came in Sight, the Fish immediately made off.

Our Author thinks this Objection of little Weight, because the Question is not, whether Fishes, when they see nothing, can be frightened by Sounds only.

Upon the Whole, our Author shews himself an experienc'd and diligent Naturalist, and will (if I mistake not) be allow'd to have fully prov'd the Falsity of any Assertion, that all Fish are intirely ~~mute~~ and deaf.

XV. *An Account of the poisonous Root lately found mixed among the Gentian; by the same.*

Read March 17.
1747.

THE following Account is the best I have receiv'd of the poisonous Effects of a noxious Root, lately found in a Parcel of Gentian, and exhibited for Use to several Persons instead of it. And as it is attended with such dangerous Consequences, I thought even an imperfect Relation of Facts had better be given immediately, than to expect more Circumstances, and wait so long for them, till greater Mischiefs might happen, by the Inattention of such as are constantly administering Medicines. The following Account was sent by a Gentleman of *Hambleton Parish, Buckinghamshire*; and is found to agree in general with some other fatal Instances that have happen'd since in *London*.

Mary Burges, aged 60 Years, about 5 o'Clock in the Morning, drank of an Infusion of only one Pennyworth (without other Ingredients) of supposed Gentian-Root, in half a Pint of white Wine: It is uncertain what precise Quantity she took; but in two Hours afterwards she falter'd in her Speech, had Twitchings and Convulsions of her Hands so far, that the ignorant By-standers alleg'd the poor Woman was drunk; and so left her a bed till 12 o'Clock, to sleep it out. On their Return however she appear'd much worse, was speechless, and remain'd so 3 whole Days, and did not know any body all that time. In her Illness a Purging came on, and at last carried her off.

Katharine

Katharine Woodward, aged 44 Years, took about a Tea-spoonful of the same Wine, and soon after fell down speechless, and her Limbs were paralytic near 36 Hours: After that she recover'd her Speech, but continued ill above a Fortnight, and Part of that Time her under Jaw was convulsed, and she bled both at Mouth and Nose, in the Beginning.

Mary Diggins, aged 33 Years, tasted a much less Quantity of the same Wine than the former had done; and though terrify'd at her Neighbour's bad Symptoms, she drank warm Water with Oil, in order to vomit; yet she soon stagger'd, and grew delirious, could not swallow any Solids, and lost the perfect Use of her Eye-sight a Fortnight.

The vague Reports of these, and Mr. *Pots's* Cases induc'd me to obtain the Favour of two or 3 Drug-gifts to look over some Gentian-Root, one Parcel of which had no less than a 20th Part of a Root, which at first Sight was discover'd to be no Gentian.

This Root, for which we have yet no Name, is of a greyish brown Colour externally, but it is browner, and more resinous internally: Most of that which I have seen, is about the Thickness of a Finger; tho' some is much larger and whiter; which is a Reason with several for thinking there are two Sorts of it; and indeed some Pieces emit a stronger and more nauseous Smell: But this I apprehend may be occasion'd only by a larger Quantity of Resin in them. All of them are of an acrid pungent Taste, and leave a Dryness on the Tongue.

I judg'd

I judg'd it therefore necessary to try what Effects this Root might have on Dogs, that I might thereby the better conjecture concerning them on the human Species; and though no Man has any Right wantonly to torture or destroy in a cruel manner the least Animal; yet when good Purposes are answer'd in the Whole of Things by inferior Natures yielding to superior ones, a Man may, without just Imputation to his moral Character, sacrifice the Interest of a baser Order to the Happiness of one superior.

With this Intention I decocted half an Ounce of this unknown Root, powder'd grossly in ten Ounces of fair Water, till two were evaporated; then let the Decoction stand 6 Hours. After this I gave half of it, stirring up the Powder, to a young Dog. This made him instantly foam at the Mouth; he grew sick, and vomited Part of the Dose; yet in less than half an Hour reel'd like one drunk, had Twitchings of his Limbs, and after some time the Motion of his Heart was irregular, and intermittent, though strong: He was sleepy about an Hour, but came gradually to himself in half an Hour more, and eat Victuals, which before he refus'd.

Two Days after, the same Dog took 4 Ounces of Decoction of Gentian made as strong as the former; but I discover'd not any bad Symptom from it. I us'd this Quantity, as Gentian-Root is sometimes given to that Quantity in the Practice of Physic. It is above ten Days since he took the first Decoction; and hitherto continues well.

Another Dog took above a Dram Weight of the unknown Root, finely powder'd, and mix'd with
Butter :

Butter . & instantly made him foam from the Mouth, and caus'd sudden Vomiting, and, in half an Hour, Weakness of his Limbs, and Staggering, which lasted half an Hour, and then he recover'd.

I tried to give a larger Quantity to another Dog; but it being too much like other irritating Medicines, caused so great a Vomiting, as destroy'd the Effects which a smaller Quantity had before produc'd.

One of the Dogs had some loose Stools after taking it; another urin'd plentifully.

Like Experiments have been made by Mr. *Pearce* at St. *Thomas's* Hospital, which had nearly the same Event.

Though none of the Dogs were killed by this Drug, but remain to Appearance well, yet all Apothecaries have sufficient Reason to examine very strictly their Gentian, and to reject what they find not genuine, since one of the Women before-mention'd, and a Man that I have heard, of are both dead; and since Gentian is of general Use in medical Compositions, as well as the primary Ingredient in the cordial Bitters Ladies make for their own Use.

XVI. *An Account of large subterraneous Caverns in the Chalk Hills near Norwich; by Mr. Wm. Arderon, F. R. S. comprised in a Letter from Mr. Henry Baker F. R. S. to the President.*

Read March 24.
1747-8.

AS the Inspection and Study of Nature is the particular Province of the *Royal Society*, and every Attempt to improve our Knowledge is certain of your Favour, I take the Liberty to lay before you the Substance of a Letter from my industrious Correspondent and Friend Mr. *William Arderon*, F. R. S. containing the Description of a large Vault or Cavern, extended under several Hills near the City of *Norwich*, with some Observations and Experiments made by him there.

About a Quarter of a Mile from the City of *Norwich*, on the East Side thereof, and near the Entrance of *Moushold-Heath*, is a large subterraneous Cavern, which has been formed in a long Series of Time, by the digging out of Chalk for the making of Lime. There's but one Entrance into it, whose Breadth is about two Yards, and its Height nearly the same; however the Height gradually rises, till at last it measures in some Places from twelve to fourteen Yards. But notwithstanding the Entrance is so small, the whole Area within is of such a large Extent, that twenty thousand Men might with great Ease be plac'd therein, as I believe will scarcely be doubted, when I assure you, that, from the Entrance to the furthest Part of these darksome Cells, measures full four hundred Yards; and that these Passages are frequently ten or twelve Yards wide, with Branchings
out

out on the Sides, into various Lanes and Labyrinth-kind of Windings, that every now and then open into one another; which renders it no easy Task to find the Way out, when a Person has been a little bewilder'd in these subterraneous Mazes.

Most of these Vaults are arched at Top, whereby the immense Weight, which every Moment presses on them, is well supported; a Weight no less than that of Hills, whose perpendicular Altitude above the Tops of these Arches is twenty or thirty Yards, if not much more. I have frequently, says my Correspondent, gone into these Caverns out of Curiosity; but could never perceive the least Appearance of those Damps* which are so common in Mines, and other subterraneous Places, where the Air is stagnant for want of a due Current; which should seem to be the very Case here, as there is but one Entrance into it. The Passage indeed is horizontal, and open to the West Wind; but the included Air's being free from Putrefaction, may possibly be owing to the large Quantity of Salt which the Chalk contains.

How deep or thick these Rocks of Chalk are, no one, so far as I can find, can tell; for, in sinking the lowest Wells, they have never, that I know of, been dug thro'; and consequently must be exceeding deep. The Chalk at the further End of this Cavern is so very soft, that it may be moulded with the Hand like Paste; which I take to be its original Consistence, and what it always retains, till it becomes expos'd to the Air. In the very lowest Parts

I i 2

of

* Those are commonly caused by sulphurous Vapours, which never appear in Chalk. C. M.

of these Vaults I have pick'd up several Kinds of Fossils, figur'd by marine Bodies; such as *Echini*, *Pectunculi*, common or fluted Cockle, *Belemnites*, &c. and, by diligent Search, other Sorts might perhaps be found. Sounds made beneath these arched Roofs are strongly reflected from Side to Side; so that the least Whisper may be heard at a considerable Distance. The Beat of a Pocket-Watch was heard distinctly full twenty Yards from where it was plac'd.

I visited this Place on the 1st Day of *November* last, in order to try the Temperature therein, as to Heat and Cold; and carried with me a Thermometer regulated by one of Mr. *Hawksbee's*, which I set down at the further End of these Caverns; and letting it remain there for some time, I found the Mercury rested at 52° . which comparing with the Register I had kept, was, I found, within half a Degree of a Medium betwixt the greatest Heat and the sharpest Cold we have known in this City for ten Years past; and it is very probable, if the two Extremes had been taken more exactly, the Temperature in these Caverns would be found to come yet nearer to the Medium of Heat and Cold in this Climate.

	<i>Hawksbee's Therm.</i>
The greatest Degree of Heat was <i>July</i> 18. 1746.	15
The greatest Degree of Cold was <i>Jan.</i> 9. 1740.	88
Which added together make	103
The Medium of which is	$51\frac{1}{2}$

I find,

I find, by inspecting Mr. *George Martin's* Collection, and Comparison of the Scales and Degrees of Heat with various Thermometers, that the Temperature of Heat in these Caverns coincides with that in the Cave at the Observatory at *Paris*, within one Degree; which I think comes very near, considering the Observations were made with different Instruments, and formed upon different Principles.

At the Foot of a high Hill, adjacent to these Vaults, issues out a curious Spring, whose Water I found exactly of the same Temperature with that underground; though, when the Thermometer was exposed to the open Air, it stood at 57° .

Permit me, Sir, to subscribe myself, with the utmost Truth and Respect,

London, March 15.

1747-8.

Your most obedient

humble Servant,

Henry Baker.

A terrible Thunder-Storm, *June 12, 1748*, shook the Earth to such a Degree as to throw down those Chalk-Vaults.

XVII. *An Account of some Observations and Experiments made in Sibiria, extracted from the Preface to the Flora Sibirica, sive Historia Plantarum Sibiriae cum tabulis æri incisis. Auct. D. Gmelin. Chem & Hist. Nat. Prof. Petropoli 1747. 4to. Vol. 1. by John Fothergill, M. D. Lic. Colleg. Med. London.*

*Read Feb. 11.
1747-8.*

BY Direction of the late Empress of Russia, several Members of the Royal Academy of Sciences at Petersburg undertook a Journey into Sibiria, in order to inquire into the Natural History of that Country, and to make such Experiments and Observations, as might tend to give a just Idea of that almost unknown Region, and to the Improvement of Physics in general.

Dr. John George Gmelin, Professor of Chemistry and Natural History at Petersburg, was sent at the Head of this Deputation, who, besides several of his Collegues, and some Students, had a Painter or two, a Miner, Huntsman, and proper Attendants in his Retinue.

He set out upon this Expedition in August 1733. and returned to Petersburg in Feb. 1742. after having spent nine whole Years in visiting almost every Part of Sibiria.

The Fruits of this Undertaking are designed to be communicated to the Public; and one Volume

of the History of Plants has already appeared, under the Title of *Flora Sibirica, sive Historia Plantarum Sibiriae, Tom. I. continens Tabulas Aeri incisas L. Auctore D. Joh. Geo. Gmelin. Chem. et Hist. Natur. Prof. Petropoli Typis Academiae Regiae Scientiarum 1747.* This is intended to be followed by several others, containing a not only a Description of the Plants, their *Locus natalis*, &c. but their Uses amongst the Inhabitants, so far as the Professor could get Information concerning them.

In a large Preface to this first Volume, the ingenious and indefatigable Author has given us a concise Account of *Siberia* in general; its Rivers, Lakes, Mountains, Mines, the Nature of the Soil, Fertility, &c. with several judicious Experiments and Remarks on the Altitude of the Earth above the Level of the Sea; but especially on the Qualities of the Air in that Climate; an Abstract whereof, at first drawn up for private Entertainment, was thought not unworthy of more public Notice, and is therefore addressed to the *Royal Society*.

The Country, whose Natural History D. Gmelin has collected, is of vast Extent: It is bounded by a Chain of Mountains called the *Werchoturian* and *Vralian* on the West; by the Sea of *Kamtshatka* on the East; and comprehends all those Countries that lie betwixt the *Mare glaciale*, and the Borders of the *Kalmucks* and *Mongales*; to the very Confines of *China*.

The Rivers which water this Tract are numerous; some of them large, and even receiving Streams in their Course, which in other Countries would be
looked

looked upon as Capitals themselves. The Space they measure is no less considerable. The *Faik* is the first River of Note on the Western Side. It rises under the Latitude of 54, of Longitude 78, and runs into the *Caspian* in 47 of Latitude, and 74 of Longitude. The *Irtisch* rises in the Country of the *Kalmucks*, Lat. $46\frac{1}{2}$, Long. 103; and empties itself into the *Oby*, Lat. 61, Long. 86. The *Oby* rises under 52 Lat. $103\frac{1}{2}$ Long.; and loses itself in the *Mare glaciale*, Lat. 67, Long. 86. after running a Course of near 800 Leagues, and receiving a great Number of Rivers of considerable Note. The *Jenisea* is not much less than the *Oby*. The *Selenga* takes its Rise under Lat. 48, Long. 114; runs into the Lake *Baical*, in $51^{\circ} 20''$ Latitude, with many others equally considerable, which it would be tedious to mention.

The Water of these Rivers is for the most part fresh, clear, and salubrious: In some it is a little brackish, by the Mixture of Currents from salt Lakes and Springs, which abound in many Places: They contain Fish of various Kinds in great Plenty, and mostly of an excellent Flavour.

The Lake *Baical* may deserve some Mention to be made of it, being one of the greatest fresh water Lakes yet discover'd: It extends, according to our Author, from the one hundred and first Degree of Longitude, to the one hundred and twenty-seventh, being upwards of 500 Leagues in Length, and is from twenty-five to eighty Leagues in Breadth. It is every-where deep and navigable; the Water is extremely clear; it abounds with great Plenty of
fine

fine Fish : It receives a great Number of Rivers, but the *Angara* alone runs out of it ; which joining the *Tungusca*, loses its Name ; as this likewise does, when it runs into the *Jenisea*.

Salt Lakes are common in many Parts of *Siberia* ; some contain a pure white Salt, well-tasted, and fit for Use ; which, in Summer, is chrySTALLISED by the Heat of the Sun alone, and forms a Crust on the Top of the Lake. In some, this grows so heavy as to break, and fall to the Bottom. Besides this kind of pure common Salt, which is fit for Use, there is another Sort of a bitter Taste, much resembling the *Sal mirabile*, found in several Lakes in this Country. Springs of salt Water are sometimes observed to rise in the midst of fresh Water : Our Author assures us, that he has seen several such ; one especially he observed rising thro' a Stone, in the Bed of the River *Angara*.

Before we dismiss the salt Lakes, we may just mention, that on the Banks of the River *Kaptendei*, where it runs into the *Wilvius*, are a great Number of salt Springs, which afford excellent Salt ; and that, about 30 Leagues above this Place, along the same *Kaptendei*, on the right Hand, is a Hill about 30 Fathom high, and 210 long, consisting intirely of *Sal Gem*.

There are some Lakes, which, our Author informs us, in the Memory of Man, contained only fresh Water, but are now very salt. One of this kind, about 40 Years ago, abounded with fresh Water Fish, but is now become salt, smelling strong of Sulphur, with a bitter Taste, and all the Fish are killed.

The Inhabitants assured our Author, that some fresh-water Lakes have been by degrees dry'd up, and that others have appeared, where formerly it was dry Ground; and that even some of these new-formed Lakes, which at first had no Fish in them, are now very plentifully stock'd. They have not recourse to subterranean Caverns or Passages, for a Solution of this *Phænomenon*; but assert, that Ducks, Sea-Mews, &c. that live upon Fish, carry the Eggs from one Lake to another.

In the Description which our Author gives us of the Course of Rivers, Situation of Lakes, &c. he takes notice of the Soil, its Barrenness, Fertility, &c. These are different, as it may be supposed, in the different Parts of such an extensive Climate under such Latitudes. About the Lake *Baical* is the most fruitful Tract, and thence is called the Granary of that Part of *Siberia*. They grow some little Corn about the Latitude of 61. They have made of late Trials still further; but the Success was not known.

In his Passage thro' *Siberia*, he tells us, that he could scarce think himself in *Asia*, till he got over the River *Jenisea*: Till then, he saw no Animals, but such as are common in *Europe*, at least may be seen in the Plains washed by the lower Part of the *Volga*: The Plants and Stones were of the same kind, and the Face of the Country in general, like other Parts of Northern *Europe*. But from the *Jenisea*, both to the East, North, and West, the Climate seemed to be wholly different, and as if it were enlivened with new Vigour. It is mountainous; but these Mountains are intermixed with rich
delightful

delightful Valleys, and fruitful Plains. The Animal that affords the Musk, and the *Musimon* of the Ancients, were now to be met with. Many of the most common *European* Plants by degrees disappeared, and others became frequent, which are Strangers in *Europe*. The Purity, Clearness, and Salubrity of the Waters, the exquisite Taste of the Fish and Fowl, but more especially the different Genius and Way of Life of the Inhabitants, plainly proved they were got into another Climate. This Remark our Author submits to the Consideration of Geographers.

Amongst the Curiosities of *Siberia* the Professor mentions a Place remarkable for its excessive Coldness in the midst of Summer. It is in the Province of *Jacutski*, about the middle Way to *Ochotz* along the River *Junacan*; it is called by the *Russians* *springing Ice*, by the Natives the *icy Lake*. Three other such Places occur within the Circuit of eighty Leagues.

The Provinces beyond the Lake *Baical* are mountainous, with high and wide-extended Plains lying betwixt them, which in many Places are only cover'd with barren Sand; so that in some Places one may travel thro' such Deserts one, two, or three Days together, without finding Wood enough to make a Fire, or any other Water than that of salt Springs, which are very frequent; and being dried up by the Summer-Heats, leave a saline Crust, very much resembling *Natron*, being of an alkaline Nature, with a sulphureous Smell.

The Country that borders on the Rivers *Urunca*n and *Gasimur* is extremely rich and fruitful. The

Face of the Country is delightful, and its Produce to the Husbandman almost exceeding his Hopes: But what renders it still more surprising, is, that a Country, whose Soil yields to few in Fertility, and the Beauty of its Bloom, should yet cover immense Riches in its Bosom. Here are Mines of Gold and Silver, which have long been worked to Advantage: The Veins are rich, and lie shallow; yet communicate no poisonous *Effluvia* to the Vegetables that cover them: Nor do those distinguishing Marks of Sterility appear here, which in most other mining Countries are so observable.

The highest Part of *Siberia* is towards the Springs of the Rivers *Argun*, *Schilca*, &c. about the 49th Deg. of Lat. 130th Longit. This Part is destitute of Marble and Lime-Stone, which are almost everywhere to be met with in the lower Tracts both of *Siberia* and *Russia*: No Petrifications are to be found here, either of the testaceous or crustaceous Animals: And the Veins of Ore are always found near the Surface, never entering deep into the Earth. Besides the Mines of Gold and Silver above-mention'd, Copper and Iron are found in several Places; likewise the *Glacies Mariæ* or *Muscovy Glass* is dug near the River *Mama*. Loadstones are also got in *Siberia*; and in several of the Rivers beautiful transparent Pebbles and Chrystals occur.

I shall only add, that there are some natural warm Baths in several Parts of *Siberia*, and some of them of a most agreeable Temperature; and proceed to the Account of our Author's Observations and Experiments on the Height of the Earth, &c.

Panda

Pauda is allowed to be the highest of all that Ridge of Mountains called *Werkoturian*. Our Author endeavoured to take the Height of it by means of the Barometer.

On the 11th of *December* 1742, at our Author's Lodgings at the Foot of *Pauda*, the Mercury in the Barometer, in a cold Place, but within-doors, stood at $26\frac{83}{100}$ *Paris* Measure. He then carried it up the Mountain as high as he could go, which was about one Third of the whole Height, where he hung up the Barometer on a Tree, from 9 to 11 in the Forenoon, making a good Fire pretty near it, lest the intense Cold, which sunk the Quicksilver in *De Lisle's* Thermometer to 201, should affect the Barometer, and lead him to ascribe that to Gravity, which was only owing to the Contraction of Cold.

Under these Circumstances the Quicksilver sunk to $25\frac{32}{100}$.

Hence, according to *M. Cassini's* Calculation, our Author's first Station will be 941 Feet higher than the Level of the Sea: The second on *Pauda* 1505 f. and the whole Height of this Mountain 4515, or 752 *Paris* Toises; which, added to 941 Feet, the Height of his Lodgings at the Foot of *Pauda*, makes 5456 Feet, or 909 Toises, the Height of *Pauda's* Top above the Sea; supposing the Level of the Sea to be 28 Inches, as the *Paris* Academicians have fixed it: Tho' this differs from Observations made on the Barometer at the Sea-coast of *Kamschatka* at *Bolcheretz*; where, from Experiments made for above two Years, the mean Height of the Mercury was 27 Inches, $6\frac{1}{2}$ Lines. And at *Ochotz*, during a Year's Observations, the mean

mean Height was found to be 27 Inches and about $8\frac{1}{2}$ Lines.

Hence it would appear, that the Sea of *Kamtschatka* is higher, with respect to the Earth's Centre, than the Ocean and Mediterranean; and at *Bolcheretz* higher than at *Ochotski*.

The following List of barometrical Observations, made in various Parts of *Siberia*, will shew the different Heights of the different Tracts in it.

	Feet	Toises	Inches
The mean Height of the Barometer, from a Year and 10 Months Observations at <i>Ir- cuts</i> , was	—	—	26 $\frac{38}{100}$
Its Height above the Sea will then be	1355 or 226	—	—
At <i>Selengia</i> , 1 Month's Observations,	—	—	25 $\frac{95}{100}$
Its Height above the Sea	1779 or 296	—	—
At <i>Kiachta</i> , a Town on the Confines of <i>China</i> 12 Days Observations in <i>April</i> and <i>May</i> , mean Height	—	—	25 $\frac{35}{100}$
Its Height	2400 or 400*	—	—

At

* In the Copy before me appears to be a great Mistake, either of the Printer, or in the Manuscript; it being put down in Words at Length, *his mille quadringentarum Orgyrum cum dimidia*; which is impossible; and the Number of Feet is not exact, according to other Calculations.

	Feet	Toises	Inches
At <i>Nertschia</i> , from 20 Days } Observations in <i>June</i> , }	—	—	25, $\frac{99}{100}$
The Height above the Sea	1738 or 298	—	—
At the Silver-Mines at <i>Argun</i> } 9 Days in <i>July</i> , }	—	—	25, $\frac{62}{100}$
The Height above the Sea	2121 or 353 $\frac{1}{2}$	—	—

Our Author adds several judicious Reflections upon the Time and Manner of making these Observations, in order to determine any thing with Certainty; which he has endeavour'd to keep strictly to in these Experiments; and concludes, that the Plains in some Parts beyond the Lake *Baical*, are almost as high as the Tops of high Mountains in some other Countries; Mount *Massane*, according to the *French* Geometricians, being but about 408 Toises high; which differs but little from the plain Country at *Kiachta*; which yet has considerable Mountains rising in its Neighbourhood.

From whence our Author concludes, that the Elevation of the Earth, in this Tract, above the Level of the Sea, is very great, compared with the West Part of *Siberia* and *Europe*. *

The

* M. *De la Condamine*, in his Voyage thro' the inland Part of *South America*, makes *Quito* to be between 14 and 1500 Toises above the Level of the Sea. Suppose ——— 1450
He tells us, that *Pichincha* is 750 higher — — — 750
This makes in the Whole — — — 2200 Tois.
above the Level of the Sea.

P. Martel, Engineer, in his Account of the *Glaciers* in *Savoy*, printed at *London* 1742, tells us, that the Barometer at *Genève*, by the Side of the *Rhone*, stood at 27 $\frac{2}{3}$ I. which is 656 Feet above the Level of the Sea according to *Schenzer*; and that the highest Point
of

The Air of *Siberia*; with respect to its Gravity, is, as in other Countries, the nearer the Sea the heavier; and the more remote, the lighter: So that at *Kiachta* scarce one Person in our Author's Retinue escaped without some Indisposition: They were seized after their Arrival, some with acute Fevers, others complain'd of extreme Lassitude and Dejection. It was in the Spring-Season, the Weather moderate, their Manner of living regular, nor had they been much fatigu'd with their Journey; in short, they could attribute it to no other Cause than the Lightness of the Air.

In these Provinces, *viz.* beyond the Lake *Baical*, our Author tells us, that Intermittents are seldom heard of, and Ophthalmies are endemic: But that, in the fenny Tracts which lie near the *Oby* and *Jenisea*, intermitting Fevers are very frequent.

The Coldness of the Air of *Siberia* is of all others the most remarkable Quality. In some Places it snows frequently in *September*, and not seldom in *May*: In *Jacutsk*, if the Corn is not ready to cut in *August*, which often is the Case, the Snow sometimes prevents it, and buries the Harvest all together. At *Jacutsk* the Professor order'd a Hole to be dug in the Earth, in a high open Place, on the 18th of *June*; the Mold was 11 Inches deep; below that was Sand about $2\frac{1}{2}$ Feet; it then began to feel hard, and in half a Foot more it was froze as hard as possible.

of *Mont Blanc*, measured partly by the Barometer, and where inaccessible from the Snow that covers it, by trigonometrical Operations, is 12459 Feet, or somewhat more than 2076 Toises above the Leve' of the *Rhon*; which, added to the Height of this above the Sea, makes 13115 *French* Feet, or about two *English* Miles and two Thirds.

possible. In a lower Place, at no great Distance from this, he order'd another Hole to be dug: The Soil was 10 Inches; soft Sand 2 Feet 4 Inches; below this, all was congealed; so that the Earth is scarcely thaw'd even in Summer above four Feet deep.

Our Author inclines to the received Opinion, that the Eastern Climates under the same Latitude are colder than the Western; and thinks this is confirm'd by Experiments made in different Parts of *Siberia*.

The Mercury in *De Lisle's* Thermometer often sunk in Winter in very Southern Parts of this Country, as near *Selंगा*, to near 226, which is equal to $55\frac{1}{2}$ below 0 in *Fahrenheit's* Thermometer. But the Cold is often much more intense than this, as appears by the following Experiments, made at *Kirenginski*.

Feb. 10. 1738. at 8 in the Morning the Mercury stood at 240 Degrees in *De Lisle*; which is 72 below 0. in *Fahrenheit's*. On the 20th it sunk one Degree.

At the same Place in 1736.

Decemb. 11. at 3 in the Afternoon 254 in *Delisle*.
Almost 90 below 0. in *Fahrenheit*.

Decemb. 20. 4 o' Clock p. m. 263 in *Delisle*.
 $99\frac{44}{100}$ below 0. in *Fahrenheit*.

	D.	F.
Novemb. 27. 12 at Noon	270	$= 107\frac{73}{100}$ below 0.

Jan. 9.	275	$= 113\frac{65}{100}$
---------	-----	-----------------------

1735 Jan. 5. 5 in the Morn. 260

6	—	280	$= 120$
---	---	-----	---------

8	—	250	and rose by degrees
---	---	-----	---------------------

till 11 at Night, when it stood at 252.

L 1

Such

Such an Excess of Cold could scarcely have been supposed to exist, had not Experiments, made with the greatest Exactness, demonstrated the Reality of it.

During this extreme Frost at *Jenisea*, the Magpies and Sparrows dropp'd down as they flew, and to all Appearance dead; tho' they most recover'd when brought into a warm Room. This was quite new to the Inhabitants of that Country; tho' it frequently happens in *Germany* in much less intense Cold, when the Weather sets in at once very severe.

The Air, says our Author, was at that time extremely unpleasant; it seemed as if itself was froze, being dark and hazy; and it was scarce possible even to bear the Cold in the Door-Way for three or four Minutes.

These Experiments, our Author assures us, were made with all possible Exactness, and agree with many others, made in different Parts of *Siberia* by his Direction; and from these we may conclude that the Cold in *Siberia* is more intense than it has yet been found to be in any other Part of the World.

It was not apprehended that a greater Degree of Cold existed any-where, than that artificial one produced by *Boerhaave*, by means of concentrated Spirit of Nitre, which sunk the Mercury 40 Degrees below 0. in *Fahrenheit's*; which was supposed to be the Point beyond which no Animal could bear it.

But the utmost Limits of Cold are yet unknown; or to what Degree an Animal can subsist in it, when inured to it by little and little. The History of Heat is alike imperfect. The celebrated Professor
- above-

above-mention'd was induced to think, that a Man could not bear, without the utmost Danger, a greater Heat than that which would raise the Mercury to 90 in *Fahrenheit's*; but an ingenious and accurate Correspondent of our Author's at *Astrachan* informs him, that it not only rises there to this Degree frequently, but even to 100, and he has seen it $103\frac{1}{2}$. Even in the Bagnio's in *Russia*, the Heat is often equal to 100: It sometimes makes the Quicksilver ascend to 108, 10, and to 116, as may be tried every Day; and yet People not only bear them with Impunity a few Minutes, but often stay half an Hour or an Hour.

One necessary Observation our Author makes, which is, that the Ball or Tube containing the Mercury ought to be as dry as possible on the Outside, during these or any other Trials with the Thermometer: For the adhering Moisture, by forming a cooler Atmosphere around it, has sometimes occasion'd a Difference of 10 Degrees.

These are some principal Facts given us by our Author in his Preface, relative to the Natural History of *Siberia* in general: What follows chiefly regards the Work it is prefixed to.

As a just Idea of this Part cannot be exhibited in a narrow Compass, the Curious in this Branch of Science must be referr'd to the Book itself.

I have only to acknowledge with Gratitude the Instruction and Entertainment I have received from this elaborate Work: It is a Tribute justly due to the learned and ingenious Author, in Return for the Pains he has taken, and the Fatigue he has endured in this inhospitable Region; and to intreat

your Indulgence, if I have flatter'd myself too much, in apprehending this Excerpt might afford you some Amusement.

XVIII. Novum reique medicæ utile *Electri-*
citatis inventum exponit *Joannes Henricus*
Winkler, Profeffor *Lipſienſis*, et *Societatis*
Regalis Londinenſis Sodalis.

Lipſiæ, die Martii 12, 1748.

Read March 31.
 1748.

SUbtilliter dividendi vim habet Electri-
 citas. Quas vero ſolvit materias,
 earum partes ſecum abripit, et in loca transfert, in
 quibus ſcintillæ electricæ exiſtunt. Res odoras in
 vitreis vaſis bene naviterque concluſas et munitas
 ita diſcerpit, ut oriundæ exhalationes æque facile, ac
 vi magnetica, vitrum penetrent, et per atmophæ-
 ram cylindrorum et catenarum, quibuſcum electrici-
 tas communicatur, inſtar fluminis dimanent. Quæ
 ex altera cylindri extremitate egreditur, materia
 electrica accedentem manum odore aromatico in-
 ficit. Non autem perſtat odor communicatus in
 hac corporis parte, quam electricum flumen afflavit :
 ſed, continuata adſpiratione, odorifera materia uni-
 verſum corpus humanum pervadit. Non modo cu-
 tis et veſtimenta fragrant, ſed aer, quem pulmones
 reddunt, et ſaliva, et fudor hominis imbuti redo-
 lent aromata, quæ in vaſe obturato electricitate agi-
 tata ſunt.

Inopinatæ huic virtuti fidem faciunt obſervationes
 et experimenta, quæ ſenſu animoque attento capta
 ſunt.

sunt. Anno 1747, lagenam vitream aqua implevi, in eaque nitrum solvi. Immota stetit hæc lagena per aliquot hebdomadas. Limpida igitur facta erat aqua, postquam nitri partes graviores fundum petierant. Sub finem anni in hanc limpidam aquam immisi filum metallicum, idque cum aliquo tubo metallico ex filis fericis suspenso conjunxi. Sub isto tubo diversis temporibus jam metalla, jam vasa metallica aquis repleta, in quibus sphaera vitrea minutis metallicis impletæ locum habebant, collocavi. His adornatis, excitavi electricitatem. Tetigit ignis electricus supposita corpora. Repetii electricitatis agitationem per complures dies. Tum vero in metallis et vasis, quæ sub tubo metallico iacta fuerant electrico igne, præter opinionem deprehendebam partium nitrosarum varie contextarum magnam copiam. Plura adhuc in conclavi, ubi experimenta institueram, vasa posita erant, quæ vero electrica materia ex tubo metallico non percusserat. In his nullum erat vestigium nitri. Ex quibus facile conjectu est, ex aqua nitri partes electricitate abripi, derivarique in loca, quæ igne electrico feriuntur.

Sub anni præsentis 1748 initium Venetiis literas accipiebam, quæ hanc conjecturam maxime confirmant. Auctor literarum, *Joannes Daniel Gaïsel*, rem narrabat, quæ *Venetiis*, *Bononiæ*, aliisque in urbibus *Italiæ* doctissimorum summorumque virorum animos excitavit. Adjecta erat epistola *Italica*, typisque expressa *, quam *Jo. Franciscus Pevati*,
vir

* Lettere sopra L' Electricità principalmente per quanto spetta alla Medicina. In *Venezia* appresso *Simone Occhi*, con Licenza de Superiori 1747.

vir juris scientia præstantissimus, reipublicæ *Venetæ* revisor, et typographicorum inspector supremus, contextuit. In hac epistola, quæ de electricitate medica inscribitur, clarissimus *Pivati* mirabilitatis plenorum effectuum historiam *Academiæ Bononiensis* secretario *Francisco Mariæ Zanotti* exponit. Artem vero, qua, quæ tradit, effecta sunt, ipse reperit atque adhibuit *Pivati*. Manifestum virtutis suæ exemplum electricitas in balsamo *Peruviano* edidit. Hic in cylindro vitreo ita inclusus atque abditus latuit, ut, antequam electricitas adhiberetur, per vitrum omni cura obseptum nihil transmitteret odoratui obvium. Hac in custodia cum esset balsamus, ad cylindrum vitreum accedit homo, qui costæ alicujus dolore affectus, suasu medici hyssopum parti morbidæ applicuerat. Fricatur cylindrus, excitatur electricitas, imbuatur eadem ægri corpus, afflictus domum discedit, somnum capit, sudorem emittit, balsamique vim dispergit. Vestimenta, lectus, cubiculum, odorem balsami spirant. Somno recreatus capillos pectit. Hos vero balsami vapor ita penetraverat, ut pecten suavi odore inficeretur. Postridie sagacissimus *Pivati* hominem bene valentem hujus rei proisus ignarum eadem, quam pridie ægrotus acceperat, electricitate implet. Hic inscius consilii, quo eum electricitatem subire jusserat *Pivati*, relictis hujus ædibus, post horam dimidiam, cum in sodalitis versaretur, teporem sentit sensim per totum corpus se diffundentem. Vigescit, et, præter melancholicam corporis sui temperaturam, hilaritate movetur. Inter quos agit, sodales odorantur miranturque suavitatem certam nescii, unde existat. Is ipse, ex quo spiritus illi jucundi effugiunt, corporis sui odorem sentiscit,

et

et miratur quid causæ subfit, incius plane virium, quibus ex vitreo cylindro sapientissimi *Privati* impletus accesserat.

Portentosæ hujus rei expositione vehementer excitatus certas statim experiri cœpi materias, in quibus electrica virtus cieri potest. Facta pericula veritatem comprobarunt. Contritum sulphur immittebam in sphæram vitream ita operculatam et oblitam, ut ex ea super igni versata nihil sulphuris odorati occurreret. Sphæra refrigerata, adhibebam electricitatem. Protinus sulphurei vapores prodibant, qui electricitatis continuatione ita refeciebant aerem, ut ad decem pluriumque pedum distantiam nares ferirent. Amicum quendam, in re electrica apprimè versatum, professorem philosophiæ extraordinarium *Hauboldum*, aliosque homines suscepti negotii partim rudes, partim conscios advocabam testes et iudices, qui vero graveolentia sulphuris statim abigebantur. Ego vero aliquanto diutius atmosphæræ sulphuratæ immoratus foetore abundabam. Vestes, corpus, et ipse spiritus oris foetebant. Imo die post sulphur olebam: quin, instituta repetitione, cum me convenisset virium sulphurearum peritus, fiebat, ut tertio die signa inflammati sanguinis in ore conspicerentur. Posthæc molitus sum effectiorem odoris jucundi. Replevi sphæram vitream cinnamomo. In quo eadem, qua dixi, cura et ratione circumsepto cum electricitas vires suas experiretur: adstantibus halitus cinnamomei occurrebant paucotempore ita augescentes, ut per universum conclave dissipati intrantium nares statim occuparent. Ad posterum diem conclave aromaticum odorem servavit. Pari successu balsamum *Peruvianum* tentavi. Amicus nominatus, cujus

cujus testimonio carere nolebam, postquam electricitatis adminiculo vim balsami conceperat, tantum odorem spiravit, ut per plateas digressus coenatum convivarum naribus negotium facessens interrogatus fuerit sæpius, quid odoraminis haberet. Ego postridie, cum potum Théë gustarem, insolita saporis suavitate afficiebar. Dimota suspitione, qua ductus de admisso aromate percontabar, ex reliquis in ore spiritibus balsaminis imbui saporem cognoscebam. Paucis interjectis diebus tentamen redorsi a vitrea sphaera, in qua balsamus *Peruvianus* conclusus nihil omnino exspirabat, catenam in conclavi extensam per fenestram libero aeri commisimus, ex eoque in conclave a priori prorsus sejunctum produximus. In hoc suspensam ex filis sericis catenam in manus tradebamus homini extenso reti serico superstanti, nostri que instituti plane rudi. Commotis aliquamdiu electricitatis viribus homo tenens catenam interrogatus, numquid subodoraretur, nares intendens annuebat; quo vero nomine nuncuparet odorem, nescire se profitebatur. Per horæ quadrantem continuatis commotionibus electricis, ita olebat conclave hoc, ut homo, cui de balsamo nostro nihil constabat, dulci odore, qualis in balsamo certo reperiretur, nares suas impleri diceret. Ex somno, quem in domo ab isto conclavi longe dissita coepit, mane surrexit admodum alacris, et ex potu Théë gustato saporem solito gratiorem percepit.

His pensatis, non dubito, quin, si quid auxilii petere posse medicinam ex vi electrica existimem, opinione duci videar probabili. Quæ ab arte salutari expectari potest, utilitatis duo præcipue sunt. capita. Aut enim res notivæ, quæ sanguini cæterisque corporis

poris humoribus immixtæ sanitati officiant, segregandæ sunt atque expellendæ: aut salubres, quæ ad tuendam firmandamque valetudinem profunt, inferendæ et distribuendæ. In utroque genere adjutrix adhiberi potest electricitas. - Hæc enim simul ac corpus humanum tangit, e momento hoc ita permeat, ut nullus in eo locus sit in quo non versetur. Quod compluribus indubiisque experimentis pater. Vi autem tanta pervadit, ut, quod in corporibus volatile effici potest, hoc non solum solvat, sed etiam dissipet et secum abripiat. Nihil igitur est, quod dubitemus, sanguinem quocumque electricitas communicatur, in partes minutiores discerpi, earum quam plurimas a massa sanguinea divelli, et brevi tempore in aerem dispelli. Non retundit vim electricam sanguinis tenacitas, non cohibet avulsionem venarum firmitas, non reprimat pinguedo. Vitri coherencia, licet multo firmior sit venarum et carnis et cutis contextu, tamen impedire non potest, quominus spiritus et aromata in partes solvantur per vitri angustias avolantes. Satis igitur causæ habere videmur existimandi, electricitate effici posse, ut ex sanguine pariter ac reliquo corpore materiæ certæ fecernantur.

Sanguinem et humores corporis per electricitatem valde agitari, resolvi et attenuari pater. Novi enim feminam, cui statim catamenia profluunt, cum electricitatem subeat. Medicus quidam nomine *Thebesius* ante paucos dies mihi scripsit *Hirschbergio* in *Silesia*, sibi nuper, si electricitatem patiatur, hæmorrhagiam narium semper supervenire.

Non autem disjungendi tantum et expellendi vim habet electricitas, sed potentissima etiam est ad locupletandum sanguinem viribus, quæ in plantis et mi-

neralibus continentur. Quod ex iis, quæ de sulphure, cinnamomo et balsamo *Peruviano* exposui, satis intelligi arbitror. Electrica via nutriendi sanguinem cum in hoc, quod sanguini sine stomachi ope alimenta suggerit, a consueta medicis ratione differt, tum halitibus, qui, quod per vitrum migrârunt, subtilitate et puritate excellunt, succum vitalem ditat. Medicamenta, quæ ore accepta in stomachum ingeruntur, antequam cum sanguine misceri possunt, per multas longasque vias errare, in iisque immutari debent. Sed qui alma electricitate aguntur, spiritus sine his anfractibus sanguinem influunt. Interdum aliqua corporis pars ex eo laborat, quod viæ, per quas sanguis aut alius liquor affluere debet, adeo obstructæ sunt, ut, quæ adhibentur, remedia ad eas aperiendas aut nihil valeant, aut longo tempore opus habeant. Quam vero partem afflatu contingunt electricitate provecta spiramina, hanc perniciousè aperiunt penitusque penetrant.

Medicinæ igitur artisque electricæ conjunctione effici posse existimo novas felicesque morborum curationes, quarum exempla insignia edidit prudentissimus *Pivati* scientis exercitatieque medici consilio usus. Impeditum obstructumque fluxum sanguinis in femina aliquâ statim restituit, ita tractatis remediis, quæ adhiberi solent, ut eorum vires ex cylindris vitreis, in quibus occlusa fuere, electricitatis adminiculo corpus ægrotantis attigerint. *Pivati* curam imploravit nobilis juvenis, ex collecti corruptique in pede humoris abundantia adeo misere affectus, ut morbus operam medicorum omnem eluderet. *Pivati* vitreum cylindrum rebus congruis impletum instructumque ope machinæ electricæ fricat; electricitatem

citatem in ægrotum derivat; ex loco, in quo morbus residebat, scintillas electricas elicit, idque per aliquot minuta prima continuat. Nox sequitur, ægrotus se somno tradit, quiete fruitur dolore mitigato, evigilat, prope talum parvum sed rubrum tuberculum vider, nil nisi pruritus sentit frigido quasi humore per interiorum pedem fluente. Per octiduum singulis noctibus peringenti sudore maduit, et, exacto hoc tempore restitutus bene valuit. Post Episcopus *Sebenecensis*, *Donadoni*, cum medico suo et nonnullis amicis ad *Privati* accessit. Præsul annos septuaginta quinque eo tempore natus manuum pariter ac pedum doloribus ex longa annorum serie laborabat. Chiragra digitos ita incurvaverat, ut extendi flectique vix possent. Podagra eum ita afflixerat, ut genua flectere ægre valeret. Adeo miser erat, ut noctu somnum capturus a servis ex sella prope lectum collocata in hunc transponendus esset, pedibus ante leniter repositis. Æger senex petiit a *Privati*, ut experiretur, quid electricitas in corpore suo valeret. Modus medendi fuit sequens. Cylindrus vitreus complexus materias viribus discussoriis instructas ita agitatur, ut virtus electrica prodeat in Præsulem. Hic derepente commotiones insolitas in digitis sentit. Actio electricitatis per duo minuta prima continuatur. Opinione citius Præsul libere alacriterque utramque manum dilatat et contrahit, unum ex comitibus manu vehementer apprehendit, surgit, ambulat, manum manui allidit, sellam occupat, pedem unum supplantat, et vires suas miratur nescius quasi vigiletne an somniet. Abit ex conclave, sine manu adjutrice scalam descendit, et more valentis juvenis in lectum se confert. Paulo post

Pivati matronam sexagenariam simili modo liberavit arthritide, qua sex menses vexata fuerat. In magno tumore fuerunt digiti continuo trementes, et brachium unum convulsionibus agitaturn est. Sed post duo minuta prima, quam vires electricitatis experta erat, tremor digitorum desit. Postero die tumor ita decrevit, ut chirothecas induere, et officio manuum fungi potuerit matrona.

Hæc adeo clara sunt, ut nullus videatur locus dubitandi de auxilio, quod medicinam sibi ex electricitate comparare posse censeo. Qua in sententia antium meum confirmat suffragium, quo iudicii plenus et in re medica versatissimus *Morgagni*, in academia *Patavina* anatomiae professor, explicatum sibi a *Pivati* negotium egregie comprobavit, eumque, subministratis consiliis, impense cohortatus est ad rem medicinæ accommodatam generique humano fructuosissimam novis subinde laboribus perficiendam.

XIX. *A Letter from Mr. Henry Baker F. R. S, to the President, concerning several Medical Experiments of Electricity.*

S I R,

Read March 13.
1748.

THOUGH perhaps as many curious and well-contrived Experiments have been made in *England* as in all the other Parts of *Europe*, to discover the general Laws and Properties of *Electricity*; we have not hitherto attended to the Effects that may be thereby produced in the Bodies of living Animals, any further than
to

to assure ourselves they may be killed thereby; a Supposition that Diseases may be cured by means of this Power, having met with so little Countenance amongst us, that very few Trials have been made, to ascertain what, in distemper'd Cases, it can or cannot perform. *Foreigners*, on the contrary, seem fond of believing, that the subtil electric Fluid (be it Fire, *Æther*, or whatever else) which can pervade all Bodies, and (being accumulated) even kill an Animal, in certain Circumstances, and by certain Methods of Application, may, possibly, in other Circumstances, and applied in different Degrees, and by different Methods, so operate on the Fluids or Solids, and perhaps on both, that very beneficial and salutary Effects * may result therefrom.

With this View the Abbè *Nollet* made several Experiments on living Birds, Kittens, and human Bodies; and if we may give Credit to the Accounts thereof communicated to us, he found, in every Trial, that *Perspiration* was so considerably promoted thereby, as to cause a very sensible Difference between the Weight of such Animals as had been electrified, and others of the same Kind that were treated exactly alike in every respect besides: Whence he naturally concludes, that, in Cases where it is necessary to quicken the Circulation of the Fluids, and throw off a greater Quantity of the perspirable Matter, *Electricity* must be greatly useful.

The

* As is suggested by Dr. *Mortimer* in these *Transact.* n. 476, p. 479. G. M.

The Philosophers in *Italy* and *Germany* have applied their Industry to discover by Experiment, how far Electricity may, simply and in itself, be of Service in several Diseases, and likewise how far it may conduce towards conveying the more subtil and active Effluvia of useful Medicines, either into the whole Body, or into some distempered Part.—Mr. *Watson* read, last *Thursday*, before the *Royal Society*, an Abstract of the preceding Paper, sent to Dr. *Mortimer* from *Leipsic*, by Professor *Winckler*, of several Experiments to this Purpose, made at *Venice* by M. *Pivati*, and repeated afterwards by himself at *Leipsic* with the same Success. He gives Instances of saturating, by Electrification, with the *Effluvia* of Balsam of *Peru*, and of Sulphur, so as to produce very remarkable Effects; and of taking a Fit of the Gout away intirely, by conveying into the Part afflicted the sanative *Effluvia* of warm and discutient Drugs.

My ingenious Friend Dr. *Joseph Bruni*, one of the principal Physicians at *Turin*, and Fellow of our *Royal Society*, has likewise sent to me an Account, lately received by him, of Experiments made at *Rome*, and at *Bologna*; which I now, Sir, lay before you, in order to shew what Attempts to the same Purpose have been made in different Countries, and by different People.—The Doctor informs me, that at *Turin* they have repeated, with great Success, the electrical Experiments made in *England*, whereof I had sent him printed Accounts; that People all over *Italy* are busily at Work making electrical Experiments; and that, at *Bologna*, the electrical Power has been applied to the Cure
of

of Diseases. He then gives me a Transcript of an Account sent him from thence in the *French* Language, which, translated, is as follows.

A Man, who had been for a whole Twelvemonth deaf of one Ear, with a continual Noise in it like the Running of Water, attended with most violent Pain whenever he lay with that Ear uppermost, coming to Dr. *Verati* for Advice, the Doctor electrified him, bringing out Abundance of fiery Sparks around the distemper'd Ear; which, in about five Minutes that the Electrification was continued, became as red as if a blistering Plaister had been applied to it. But the Redness disappeared in a few Minutes after, the Patient passed the Night with less Pain and Noise, and was perfectly cured of his Disorder.

A Footman belonging to the said Doctor, being taken suddenly ill of a violent Pain in the Head, which continued many Hours, he was thereupon electrified, the Doctor causing the Sparks of Fire to issue from the Temple wherein the Pain was felt. The Part appeared red, the Pain abated; in three Hours it was intirely gone, and has never returned since.

A Woman that nursed one of the Doctor's Children, having had a most grievous Disorder in her Eyes for some Months, with a continual Running of Water from one of them, and a constant Pain over the Eye-lid, came to the Doctor for Advice; who immediately electrified her, bringing out the
fiery

fiery Sparks about the Eye and Eye-lid, whereby the Eye appeared very much blood-shot; but that went off in 7 or 8 Minutes. The Woman felt less Pain the following Night, and opened her Eye in the Morning more easily, and without being obliged to wipe it, as she did before: The watry Humour and Pain were much diminished; and the Doctor hoped, that, by repeating the Operation twice more, he should be able to cure her quite.

Dr. *Bruni* gives me next his Information from *Rome*; which is, that a Gentleman there cover'd the internal Surface of a Cylinder of Glass (which some use instead of a Globe) with a purgative Medicine; and that a Man, electrified therewith, found on the Spot the same Effects as if he had swallowed the Medicine. He then recommends to us in *England* to try how far the electric Power may be of Service in Distempers.

These Cases, Sir, and particularly the last, as it may to some appear extravagant and whimsical, I should have been cautious of bringing before the *Royal Society*, had you not judg'd it proper they should be added to those similar Accounts from other Places which were read to us last Meeting. I think neither myself nor Dr. *Bruni* answerable for the Truth of these Facts, as we relate no more than what we have received. In Truth, all the *Phænomena* in Electricity are so wonderful, that it is scarcely prudent to deny the Possibility of any Accounts concerning it, till we have made Experiments carefully ourselves. We are *very sure* it is possible to
render

render a living Body replete with electrical *Effluvia*, or to transmit and send such *Effluvia* through a living Body, in a Stream, as long as we think proper: We are *not sure* that it is impossible for these *Effluvia* to convey with them into that living Body the most subtile and active *Effluvia* of other Substances; and if they can do so, the Effects suggested are not wholly improbable; for several Experiments have proved, that a very minute Quantity of Medicine, transfused directly into the Blood, and circulating Fluids, will have the same Effect as a large Dose thereof taken into the Stomach. Therefore even this last Case, romantic as it may seem, should not be absolutely condemned without a fair Tryal; since we all, I believe, remember the Time, when those *Phænomena* in Electricity, which are now the most common and familiar to us, would have been thought deserving as little Credit, as the Case under Consideration may seem to do, had Accounts of them been sent us from *Rome, Venice, or Bologna*, and had we never experienced them ourselves.

I am proud to seize every Occasion to assure you with what great Respect I am,

S I R,

Strand, March 28,
1748.

Your most faithful and
obedient humble Servant,

Henry Baker.

ERRATA.

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I. *A Proposal for Checking in some Degree the Progress of Fires; by the Rev. Stephen Hales, D. D. & F. R. S.*

Read April 21. 1748. **T**HE late destructive Fire in *Cornhill* bringing to my Mind what I thought a probable Means to check, in some degree, the speedy Progress of Fires (which, if it could be effected, would be of great Importance I made the following Experiment, in order to form a more certain Judgment of the Matter; *viz.*

I placed on two Garden-Pots a dry Fir-Board, which was half an Inch thick, and nine Inches broad; and cover'd nine Inches Length and Breadth of it with an Inch Depth of damp Garden-Earth; fencing this Earth on each Side with two Course of Bricks, in order to make a Fire-place to contain the Wood-Fuel and live Coals; which were frequently blown with Bellows, in order to keep the Fire to a vigorous Heat: This was done for two Hours Continuance, before the Fir-Board was burnt thro'; when there was only a weak lambent Flame at the under Part of the Board; for it could not flame out for want of proper Fuel; because the Substance of the Board was reduced to a brittle Charcoal, by the Heat of the Inch-Depth of Earth which lay on it, which hinder'd the burning Board from flaming. And it was observable, that the Edges of the Board burnt only with a live Coal like a Match; being hinder'd from flaming, by the Earth which lay on the Board.

May it not hence be reasonably inferr'd, that, when a House is on Fire, it may be a probable Means considerably to retard the Progress of the Fire, to cover with Earth the Floors of the adjoining and more distant Houses, which stand in the Course of the Progress of the Flames ?

The thicker the Earth is laid, so much the better: But if Time will not permit to lay it more than an Inch thick, then supposing 27 Men to carry each a cubic Foot of Earth, which will be a cubic Yard of Earth; then that cubic Yard of Earth will cover 36 square Yards of Flooring; which repeated several times, would soon cover all the Floors of a House. And as the Fire probably mounts with great Fierceness up the Stair-Case, it will be well to lay much Earth on the Stairs; which will help to give some Check, especially, as the Earth on the Floor and Stairs may be wetted by the Fire-Engine; which Moisture will be much the longer retained by means of the Earth; whereas Water, when not thus retained, soon glides away.

And as Fires often catch from House to House at their upper Parts, an upper Floor cover'd with Earth, with the Rafter's burning on it, will be longer in burning to such a degree as to fall on the next Floor, so, when fallen there, it will also be the longer in burning, and will flame the less, on account of the Earth on that next Floor; and, consequently, will not be so apt to fire the next House, as in the common Case of Floors without Earth, which must needs therefore burn the more fiercely.

Earth may be had either in back Yards, or Cellars, or Streets.

These

These Hints, from one who never saw a House on fire, will, 'tis hoped, be farther improved by those who have more Experience and Skill in these Affairs.

II. *Some Observations, made during the last three Years, of the Quantity of the Variation of the Magnetic Horizontal Needle to the Westward; by Mr. Geo. Graham, F. R. S. at his House in Fleetstreet, London.*

Read Apr. 21.

1748.

1745	March 26	- - -	17 = 0
	29	- - -	17 = 0
	March 18	- - -	17 = 10
	21	- - -	17 = 10
	April 22	- - -	17 = 15
	May 4	- - -	17 = 18
	14	- - -	17 = 20
	16	- - -	17 = 15
	Dec. 18	- - -	17 = 25 $\frac{1}{2}$
	Febr. 24	- - -	17 = 30
1747	Dec. 19	- - -	17 = 40
	Jan. 4	- - -	17 = 40—

The Inclination of the dipping Needle has been during the same time about $73\frac{1}{2}$ Degrees.

N. B. As the Variation of the Needle at London has not been regularly published from time to time in the Philosophical Transactions: It may not be improper to take notice here, that according to the best Observations extant, and which were made by Persons of great Skill and Exactness, the Needle at London declined to the Eastward $11^{\circ} 15'$ in the Year 1580. In 1657 there was no Variation, the Needle then pointing due North. In 1672 the Variation was observed by the late Dr. Halley $2^{\circ} 30'$ towards the West, and in 1692 $6^{\circ} 0'$. And towards the Beginning of the Year 1723, it was found by Mr. Graham, from the Medium of a vast Number of Observations, to be then $14^{\circ} 17'$ the same Way. So that, during the Course of 167 Years elapsed since the Year 1580, to the End of the last Year 1747, the magnetic Needle at London has moved to the Westward, $28^{\circ} 55'$. See before N^o. 148, and N^o. 383 of the Philosophical Transactions.

III. *A Letter from the Right Hon. John Earl of Orrery to Martin Folkes Esquire, Pr. R. S. inclosing an Account of the Cornel-Catterpillar, contained in a Letter from the Reverend Mr. Philip Skelton to His Lordship.*

S I R,

Read April 21.
1747.

THE great Honour which I have received from the *Royal Society*, makes me very ambitious of expressing my Sense of it, in some manner that may at least be a Mark of my Gratitude, however unworthy of their Attention. Inclosed in another Packet I send you a Letter I have received from the Reverend Mr. *Philip Skelton*, who, at my Request, has drawn up the Account of a sort of Caterpillars, that appeared very numerous in these Parts some Years ago. He has likewise pasted on the first Leaf a Piece of their Web*. He is my Neighbour here, and a Gentleman of Sense and Learning. The Letter is so full and explicit, that I need say nothing on the Subject: Unless hereafter you are desirous to know further Particulars; and I hope you are convinced, that I shall be always ready to obey your Commands. I am, Sir, with the greatest Esteem, and the utmost Respect,

Calidon, April 6th 1748.

Your most obliged and

obedient humble Servant,

J. ORRERY.

The

* To be seen in the *Museum* of the *Royal Society*.

The Rev. Mr. Skelton's Letter to the Earl of Orrery.

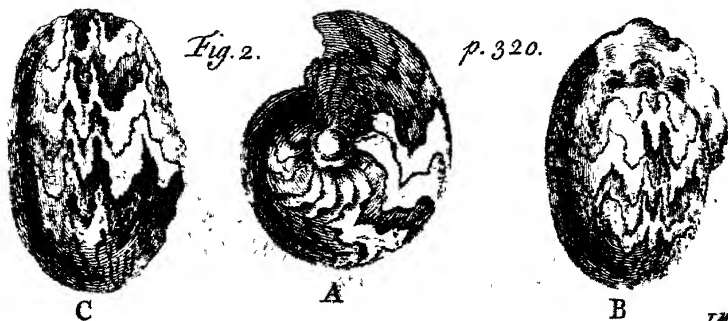
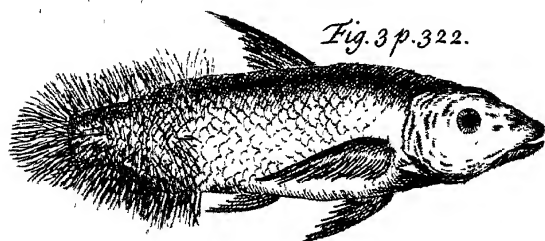
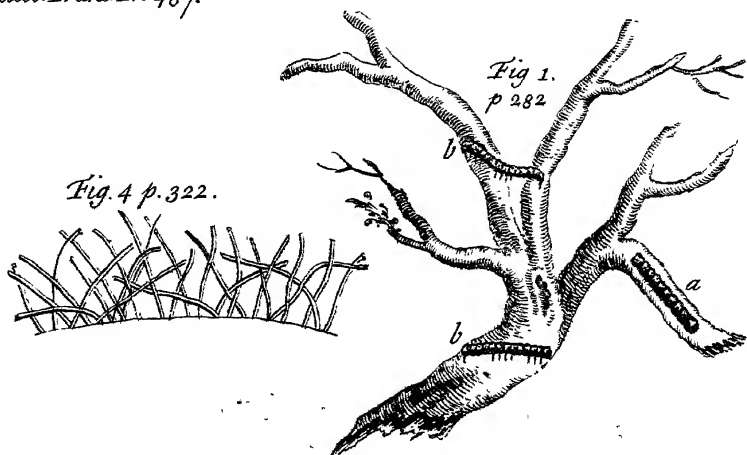
Monaghan, March 29, 1748.

MY LORD,

PURSUANT to my promise, I send you an account of the Cornel-Caterpillar; the web it produces, the flie into which it is changed; and a small sample of its work.

In the beginning of *May* 1737, the warmest season that any body now alive remembers to have felt, the Cornel-trees, of which we have a good number about this place, appeared almost cover'd with small Caterpillars of the size and shape in *TAB. I. Fig. 1.* and in general of a duskyish green, resembling in colour the bark of the tree, altho' a few, considerably larger than the rest, were yellow. These worms were employed partly in feeding on the leaves of the Cornel, which was their only Nourishment, and partly in crawling (with a very swift motion for a worm) over the bark of the tree. As they crawled, they left each a fine thread, scarcely visible to the naked eye, sticking to the bark. These threads, being almost infinitely multiplied by the inconceivable number of worms employed in the work, formed the web, in which the threads are not interwoven, but cohere by some roughness or glutinous quality.

By the end of *May* there was not a leaf to be seen on any of the Cornels, excepting a few, reserved for a very curious purpose, which I shall have occasion





occasion to mention presently. But the worms, in the room of the green cloathing they robbed those trees of, gave them one of white, so entire, that it covered the whole bark, from the ground to the points of the slenderest twigs, and of so pure and glossy a colour, that the whole tree shewed in the sun as if it were cased in burnished silver. The web was so strong, that if one disengaged it from the tree, near the root, one might have stripped it from the trunk, the branches, and the twigs, at one pull. As soon as the worms had covered all the *Cornel-trees*, they removed from thence, and covered all the *Ash*, *Beech*, *Lime*, *Crab-trees*, and even weeds, that grew near them, with the same, but a thinner, kind of workmanship.

Perhaps, my Lord, you will desire to know how they travelled from one tree to another. Many of them crawled along the ground, and over every thing in the way, still leaving a thread behind, and dispatching a part of their business as they went to a more convenient surface to finish the rest on. But I really imagined some of them took an easier and more ingenious way. I found many of them hanging by their own threads from the most extended branches of the tree. While they were in this situation, a gentle puff of wind might, by exciting a pendulous motion, waft them to the next tree. This seems to be the method, by which those very minute spiders, whose threads are made visible by the moisture adhering to them in a foggy morning, transport themselves from one bush to another, altho' destitute of wings, sometimes across narrow paths, and even rivulets.

As

As the worms, neither while they were working, it, nor afterwards, made any use of the web thus left on the bark of the trees, I take it for granted, they wrought for no other purpose than to rid themselves of that glutinous mass, out of which it was spun, and which, nature producing it that season in greater abundance than was necessary for the wrapping and stowing the worm in its *Nympha* state, prompted the creature to work off the redundancy the best way it could. The method it made use of for this purpose was very well judged. It fasten'd its thread to some little eminence on the bark; and chusing, for the greater convenience of crawling, that even surface, kept continually in a brisk motion, till the troublesome superfluity of its burthen was discharged. I can but guess at its reason for removing from its own native tree, to spin abroad upon the neighbouring ones. Perhaps it found the web too bright for its eyes, or the threads, already layed, might have stuck to its feet; for your Lordship may observe that the web is very apt to stick to the fingers, when it is touched.

About the beginning of *June* the worms retired to rest. Their manner of preparing for, and executing this, was very ingenious and curious. Some of them chose the under sides of the branches, just where they spring from the trunk, that they might be the better defended from the water, which in a shower, flowing down the bark of the tree, is parted by the branches, and sent off on each side.

Here they drew their threads across the angle, made by the trunk and branch, and crossing those again with other threads in a great variety of directions

rections, they afterwards formed a strong tegument on the outside. Within this they placed themselves lengthways among the threads, and rolling their bodies round, spun themselves into little hammocks of their own web, while in the mean time they shrunk into half their former length. Those hammocks, being suspended by the transverse threads, did not press each other in the least. That they might take up the less room, they lay parallel to one another, and in the most convenient order imaginable.

Others, still more ingenious than these, fastened their threads to the edges of certain leaves, which, no doubt of it, they had saved from their stomachs for this very purpose; and with that slender cordage pulling in the extremities of the leaves, drew them into a kind of purse, in the inside of which they formed the same kind of work, and laid themselves up in the same manner as above. By this method they saved themselves a labour, which the rest were at the expence of; for the leaf served them very well for an outward defence against the weather, and a place to fix their transverse threads to. It is probable they laid themselves up in great numbers together, not only because many were necessary to the work of providing a common covering, but also to keep one another warm, while nature was preparing for the great change, and also to confine some subtil vapour, issuing from their bodies, which might have been conducive to their reviviscence, and which had been easily dissipated, had they not lain close, and caught it from one another.

Between the worm, thus laid up, and the hammock, in which it was enclosed, a tough and pliant shell, of a dark-brown colour, was found. This I take to have been formed by the perspiration, or rather by some glutinous stuff, forced through the pores of the insect, while it was contracting itself, which being stopped by the close texture of the hammock, consolidated, and formed an interior covering for this delicate creature. As the worms themselves were of a pretty dark colour, this superficial tincture seems to have been in a great measure purged off into the shell.

For after the worms had continued in this state during the whole month of *June*, whether they gnawed their way through the ends of their shells and hammocks, or that exit was prepared for them by some corrosive matter oozing from their mouths, I know not, but they came out almost all in the space of one morning, the most beautiful fly or moth that my eyes ever beheld. Its shape was extremely elegant; its head, upper wings, body, legs, and *antennæ*, were of the purest white, and glitter'd as if they were frosted with some shining kind of substance. I rubbed some of this off, and upon viewing it thro' an ordinary microscope, it appeared like the points of very minute feathers, or like small cones of polished silver. The upper wings were regularly studded with small, round, black spots, and extended themselves from its head somewhat beyond its tail. The under wings, which were a little shorter, were of a duskyish colour, and prettily fringed at the extremities.

This

This beautiful and surprising work of nature seemed, after its resurrection, to have no dependence on material food. The Cornel had recovered a new set of leaves by the time the fly appeared; but it never touched them; and those that came out in my room, lived as long there, as the rest which enjoyed the open air, and the tree on which they were bred. If they did feed, it must have been on some other adventurer of the air, too minute to be visible to our eyes. Those that were confined to my room, discharged a small drop of brown liquor, in which I suppose their eggs were contained; but as they were not deposited in a proper receptacle, they did not produce worms the next year. As the tree is the peculiar habitation of the worm, and supplies it with its only food, so it is certainly the only nurse of its egg. It is likely the eggs were either inserted into the small crevices of the bark, or discharged into the little apertures, where buds are to spring the following season. In this situation, they might be most conveniently nourished by the return of that genial juice, or spirit, with which the Cornel is naturally fitted to cherish and raise them into life. The flies seemed to be of a most delicate constitution in respect to heat and cold. The former they could bear with difficulty; the latter, not at all. Hardly any of them survived the first of *August*. They loved rest, and did not care to flutter much about. While they were yet in their *Nympha* state, I brought great Lumps of them to my room, and those, which happened to be bruised in pulling them from the trees, produced flies, distorted either in the wings or other parts; but this

Distortion generally wore off, in a little time, and the pretty creature recover'd its own natural symmetry of shape.

In the beginning of *May* 1738, they began to work again in prodigious numbers, and having covered some trees, were stopped, and most of them destroy'd by the foul weather that followed. Their web also was smutted and discolour'd. I send your Lordship a piece of each year's produce. The whitest is that of 1737, the other of 1738.

In 1739 they appeared in small numbers, and much shrunk in their Size, and wrought only sufficient covering for themselves.

They appeared again in the Year 1740; but it was plain the great frost had destroyed most of their eggs, and checked the growth of those that escaped; for there were very few of them to be seen, and twelve of them were not larger than one in *May* 1737.

Every year produces more or less of them, with some small variation, as to the number and size.

The place where our Cornel-trees stand, is surrounded with steep hills, and closely shelter'd with a very thick plantation. This was probably no inconsiderable help to the prodigious encrease of this puny reptile. I verily believe both an unusual warmth of air, and a deep shade, were equally necessary to it; for I observed, that those Cornels, which stood more exposed to the cool air and the sun, abounded less with worms than the rest.

I have been scrupulously exact, my Lord, in relating the above particulars, which I did not trust

to

to my memory, but reduced to writing immediately after I had finished my observations.

This curious *phenomenon* naturally leads one to enquire, how these creatures came to breed on the Cornel-trees, and what occasioned the prodigious encrease of them at that time. Here fact gives us up to conjectures. I hope however that mine will not seem to your Lordship altogether unsatisfactory, but rather help to clear up those difficulties, and at the same time carry our eyes a little farther into nature, than merely to what concerns this species of insects.

There is not an animal, nor a vegetable, that may not be considered as a little world, in respect to the habitation and nourishment it affords to certain insects peculiar to itself. The scheme of life begins in vegetation; and wherever on the earth, or in the water, nature is able to produce vegetables, she always obliges them to pay for their elemental nourishment to certain insects, animals, or fishes, which she billets on them. These again are forced to refund to others, to diet and lodge, each of them, a set of living creatures, assigned to them by the universal scheme of nature.

This traffique of life, this just community in nature, which suffers nothing to subsist merely for itself, is found not only every-where on the face of the earth, but also in all lakes, pools, rivers, and in the ocean. By microscopes we discover a prodigious variety of little creatures, all feeding either on the floating vegetables, which that element produces in a state of stagnation, or on one another. As to the sea in particular, we know only what happens

happens about the shores, where we see vegetables of various kinds, on which a like variety of insects are bred and nourished. These, together with a prodigious number of others, bred in the mud, become the prey of the smaller kind of fishes, and they again of the greater. That this scheme of nature, in supporting life by death, found every-where else, dives into the depths of the ocean, may appear probable from the wise frugality of nature, which hath an useful end in every thing, and besides rejoices in filling the world with life and motion; and also from the wonderful kinds of fishes, which are now-and-then washed up by violent storms from the deep waters, or happen to pursue their prey, from the low lands of the ocean, to the higher grounds at the shores.

Franciscus Redi, in his curious treatise concerning the generation of insects, hath not only refuted the notion of equivocal generation, but also hath shewn us, that each animal and vegetable hath its own peculiar insects to maintain; and *Eleazar Albin*, in his collection of various caterpillars, and the butterflies, into which they are transformed, hath given us a beautiful demonstration, from above an hundred instances, that each species hath its own proper plant, to which it is by nature peculiarly adapted, and on which only it can feed, or live for any considerable time.

Now the Cornel, my Lord, is the plant, on which alone the worms, we have been speaking of, can be propagated and fed. The specific qualities, with which the juices of this tree are impregnated, fit it
for

for the propagation and support of this its native insect. If these peculiar and distinguishing qualities reside, as the chymists say, in the essential oil of the plant, it will follow, that this, as well as other insects, subsisting on vegetables, are by some means or other qualified to extract, in a nicer manner than any chymist can do, the essential oil of their respective plants, nothing else therein being of a nature sufficiently peculiar either to assist the propagation, or supply the nourishment, of the insect.

As to the difficulty, how this plant came to receive the eggs of this fly, it is as great in respect to the propagation of any other insect on its peculiar plant. The flies of every plant have continual access to those plants, and no doubt are prompted by the sight, smell, or other qualities of their native vegetable, which are congenial to them, to propagate their kind upon them. As this act is probably attended with some degree of pleasure, it keeps them continually busy in the work of impregnating their proper plant. Hence it comes, that before the younger plants are removed to a distance from those that are more fully grown, they receive sufficient colonies from others, already peopled, which they extend again to their succours, the flies each year impregnating all the plants within their reach. Whether the thing happens in this manner, or that the eggs of insects so small are minute enough to be carried through the air, and so dispersed everywhere, it is nevertheless a fact, that no vegetable is found without its insects, tho' propagated by the seed.

As

As to the extraordinary increase of this insect in *May* 1737, the succession of seven or eight mild winters, which preceded that season, might, by preserving their eggs, give occasion thereto. As they are one of the earliest kinds, the excessively warm *May* that year so effectually hatched their eggs, that they all came to perfection: Whereas the more ordinary worms and flies, that make a later appearance, meeting with the sharp easterly winds that happened that summer to blow during the months of *July* and *August*, were in a good measure destroyed; otherwise it is possible they too might have had an extraordinary increase.

However I own, my Lord, this reason hath its objections, and doth not fully satisfy me. There is scarcely a year that is not remarkable for some one kind of insects or flies, when no colourable reason can be assigned for it from any known temperament of the year, which might not as well favour a great increase of any other species. Insects, as well as fevers, are epidemical, and probably depend as much on a certain occult constitution of the air, water, or earth. Nay, it is an opinion received by some, that all pestilential disorders are nothing else than prodigious flights of invisible flies, of which each sort, according as the constitution of the year assists it, takes its turn to multiply from worms proportionably little, bred in putrid carcases, especially after great battles, and being raised from thence into the air, are waisted not only from one body to another, but even to distant countries. *Sydenham*, and, if I mistake not, others, have observed, that the seasons immediately preceding those
in

in which the plague raged, abounded unusually with all sorts of flies; which shews at least, that the constitution of the air doth at those times greatly favour the production of such creatures. This conjecture will seem the more probable, as the usual preservatives against infection, namely vinegar, tobacco, rue, wormwood, &c. are endued with very acrid and pungent particles, with which perhaps they sting and kill the invisible flies before they can lay their eggs, and by these means preserve us from contagion.

Be this however as it will, it is certain there is such a constitution as we are here speaking of, in respect both to distempers and insects. But whence this proceeds, whether from the sun alone, or from the joint influence of other neighbouring planets, or the transudations of mineral vapours, or fermentations in the soil of the earth; and further, whether this sort of climacteric in the seasons be stationary or casual; I leave better naturalists to judge.

I only insist, that such a constitutional temperament there is, which, running thro' all nature, doth at certain times give more than ordinary energy to the prolific powers of such plants or animals as are of nature similar thereunto.

This plainly appears to us in plants of all kinds, even excluding the consideration of warmer or colder, of drier or moister seasons, which, it is manifest, have only their share in the casualties to which the vegetable world is liable. They frequently bear more blossoms and fruit in a bad, and less in a good season; and what puts the matter beyond all question, that season which is favourable to one kind

kind of vegetable is prejudicial to another; whereas much heat and moisture together are equally indulgent to all. This, in my opinion, shews that each plant hath a specific vegetation of its own, as well as one common to all, and that the former depends upon somewhat else than mere warmth and moisture.

The constitution therefore of the year disposes the vegetative spirit, whether residing in the air, the earth, the water, or in all, to supply sometimes these, and sometimes those vegetables, with a greater or less proportion of aliment. By these means a greater quantity of that juice, which distinguishes any one species of plants from all others, and enables it to give life and food to its peculiar inhabitant, must necessarily be produced one year, than another; and consequently the eggs, deposited in the cavities, or perhaps in the very perspiratory pores of its bark, must be better cherished, and the worm more plentifully fed by the leaves, which in such a year contain a greater abundance of the specific juice, and that more perfectly elaborated.

From hence it may seem reasonable to rest in this conjecture, till somewhat more certain is found out, that the annual constitution being more indulgent to the vegetation of one plant than of another, promotes the growth and fertility of this, which is of a similar, and checks the increase of that which is of a dissimilar nature. The plants, thus differently supplied, furnish their respective insects accordingly. Hence again it comes to pass, that many species of insects, having been injured by some unknown disposition of the air or earth,
seem

seem almost extinct in one season, and swarm out again in another, as if there had been a new creation of them. One year, the wall-fruits are devoured by earwigs; another, we are pester'd everywhere, and even in our closest chambers, with unusual multitudes of the common fly. One year the wasp predominates; another, the gnat; and a third, the scale-caterpillar. One year, the farmer complains of a worm, hardly known to him before, that destroys his corn; and the gardener does the same another, in respect to an insect that falls greedily on his seeds, as soon as they are committed to the ground. The *African* locusts come some years into *Spain* in such swarms, that they cover the face of the earth; and when they have devoured the whole herbage of the country, retire again to their own, and do not visit *Spain* in the like numbers for several years. Large old orchards are some years suddenly stripped of all their blossoms and leaves, by a prodigious increase of the apple-tree-worm; and groves of oak have been served in the same manner by the caterpillar peculiar to that tree. This must needs give a check to the growth of the tree more than equivalent to the great increase promised at such a time by the extraordinary redundancy of the vegetative spirit.

I have now finished what I had to say on this surprising subject, at which some gentlemen stupidly important, may laugh, as at an affair not worthy of so much notice, and so many words; but I am persuaded, my Lord *Orrery*, who regards not things by their bulk, but their excellence, will see the wisdom and power of God as gloriously displayed in

this little insect, as in the Behemoth, or the Leviathan. It is a flight high enough for the faculties of man to rise by contemplation to a competent knowledge of the meanest work God ever condescended to form. That which was not beneath the Majesty of God to make, can never surely be beneath the dignity of a rational creature to contemplate. I am,

My LORD,
*Your Lordship's most obliged, and
 most obedient humble Servant,*

Philip Skelton.

IV. *The Extract of a Letter from Dr. James Mounsey, Physician of the Czarina's Army, to Henry Baker F. R. S. concerning the Everlasting Fire in Persia.*

S I R,

*Read April 27.
 1748.*

AS you inform me any thing relating to the Natural History of *Persia* will prove agreeable, I have some time ago wrote to a couple of Gentlemen, a Physician and a Surgeon, both Men of Learning and Veracity, and my very intimate Friends, who are now with the Ambassador from this Court to *Persia*, and they, both have promised to communicate to me whatever they shall meet with remarkable in that Country, and

and you may depend on receiving from me all the Accounts they shall please to send.

In the mean while, as the Natural History of *Persia* is but little known, and the Authors of the *Universal History* have given no true Account of the *everlasting sacred Fire* which the *Gauers* worship, I shall now send you a Description thereof, which you may depend upon, as there was a *Russian* Army for some Years in the Kingdom of *Dagestan*, where that Fire is; and I took down what I am going to relate from the Mouths and Journals of many Officers that were there, and more particularly from what was communicated to me by *Archibater Fischer*, who received an Account thereof from Dr. *Lerch*, Physician of that Army.

This perpetual Fire rises out of the Ground in the Peninsula of *Abscheron*, about twenty Miles from *Baku*, and 3 Miles from the *Caspian* Shore. The Ground is very rocky, but has a shallow Covering of Earth over it. If a little of the Surface be scraped off, and Fire be applied to the Hollow, it catches immediately, and burns without Intermision, and almost without Consumption; nor is ever extinguished, unless some cold Earth be thrown over it, by which it is easily put out.

There is a Spot of Ground, about two *English* Miles large, which has this very wonderful Property; and here is a *Caravansary*, round which are many Places where the Earth continually burns; but the most remarkable is a Hole about 4 Feet deep, and 14 Feet in Diameter. In this *Caravansary* live 12 *Indian Priests*, and other *Devotees*, who worship the Fire, which, according to their Traditions, has
burnt

burnt many thousand Years. It is a very old vaulted Building, and in its Walls are a great many Chinks, whereto if a Candle be applied, the Fire catches instantaneously, and runs instantly wherever the Chinks communicate; but it may be easily extinguished: They have hollow Places in the House fitted to their Pots, which they boil without any other Fuel; and instead of Candles, they stick Reeds into the Ground; from the Tops whereof, upon applying Fire thereto, a white Flame immediately comes forth, and continues to burn without consuming the Reeds, until they think proper to extinguish it, by putting little Covers over them for that purpose.

They burn *Lime* of the Stones dug hereabouts, first making an Hollow in the Ground, and then heaping the Stones on one another. This done, on applying Fire to the Hollow, a Flame bursts out, and is dispersed at once with a very great Crack through the whole Heap of Stones; and after it has continued burning for three Days, the *Lime* is ready: But Stones placed in this Fire for setting their Pots on never turn to Lime; which cannot be made but by heaping them on one another. The Earth and Stone are no farther warm than where the Fire reaches: And what seems very well worth Observation, this Flame of Fire gives neither Smoke nor Smell, however great it be.

About an *English* Mile and half from this Place there are Wells of *white Naphtha*; which is exceedingly inflammable; and though the Flame of *Naphtha* affords both Smoke and Smell, it is highly probable the perpetual Fire I have been describing is owing to *Naphtha*, but so purified, in filtering through

through the Stone, that it becomes divested of all such Particles as produce Smoke or Smell. The Stone and Earth are grey in Colour, and saltish to the Taste; and indeed much Salt is found on this Peninsula of *Abscheron*. There is also a salt Lake, near the Side of which the *white Naptha* flows by five different Springs. This *Naptha* is made use of only in the medicinal Way. It is yellowish from the Spring, but when distilled resembles Spirit of Wine. They give it internally, for Gonorrhœa's, Disorders of the Breast, and for the Stone; and they apply it externally in gouty Cases, Contractions of the Sinews, and Cramps.

Black Naptha is produced 8 or 9 Miles from the perpetual Fire; it is thick, and being distilled grows not clear but yellow. About *Baku* there is some of it so thick, that they employ it for greasing Wheels: But the best and greatest Plenty, is at *Balachame*, where there are above 50 Springs, the greatest whereof produces every Day 500 *Batman*, each *Batman* containing ten *Russ* Pounds, which are somewhat less than *English* Weight. You hear it make a considerable Noise in rising out of the Ground, though the Spring be 20 Fathom deep.

In *Baku* they have little or no other Fuel to burn besides *Naptha*, but it must be mixed with Earth or Ashes to make it fit for Use. The Fire it makes is only good to boil with, and this Inconveniency attends it, that all their Food so boiled smells and tastes of *Naptha*. For baking and roasting they make use of *Abrotanum*, *Absynthium*, and such-like; but in general *Naptha* is their Fire.

You

You may depend on the Truth of this Account, and I hope it will be acceptable; the Hurry I am in, being Physician to the Army now on its March to the Assistance of the Allies, and to set out from this Place To-morrow, with the Commander in chief, who has been some time here indisposed, and under my Care, prevents me from adding any more at present; but you shall be sure to hear from me, when we are advanced into *Germany*. In the mean while, believe me to be sincerely,

Dear Sir,

Riga, Feb. 24.
1748.

Your most humble Servant,

James Mounsey.

V. *An Abstract of Mr. Bonnet, F. R. S. his Memoir concerning Caterpillars; drawn up in French by Mr. Abraham Trembley, F. R. S. here translated into English.*

Read April 27. 1748. **T**HE Paper lately communicated by the *President* from Monsieur Bonnet of Geneva, contains various Experiments he has made relating to the Respiration of Caterpillars.

Malpighi first discover'd, that those 18 Openings or Orifices, which are placed 9 on each Side of the Caterpillar, and which are called by the Name of *Stigmata*, serve to give Respiration to this Class of Animals.

Monfieur

Monſieur *de Reaumur* has repeated the Experiments of *Malpighi*, and made ſeveral new ones upon this Subject. And he has been of Opinion, that theſe Apertures ſerved only for the Inſpiration of the Air, which the Caterpillar afterwards expired, through the whole Superficies of its Body. What he has wrote upon this Subject is in the firſt Tome of his Memoirs, at the 131^{ſt} and the following Places.

Mr. *Bonnet* has had Reason to think theſe Caterpillars do both inſpire and expire the Air by their *Stigmata*; and that they did not expire any of it through the Pores of their Body. This Paper here ſhewn gives an Account of 36 ſeveral Experiments, made chiefly with Deſign to diſcover this Fact, whether indeed theſe Inſects did both inſpire and expire the Air by their *Stigmata*, or only inſpire it. Theſe Experiments, like Mr. *de Reaumur*, conſiſt moſtly in the plunging of Caterpillars either into Water, or ſome other Liquor; ſome alſo they daubed or anointed over with fat and greaſy Subſtances, ſome quite over, and others only in ſome Places.

Mr. *Bonnet* is inclined to think, that the ſmall Bubbles of Air obſerved all over their Bodies, when they are immerged in Water, do not come from the Air included within them, and which they expired by the Pores; but that they are formed by the Air only lodged near the Surface of the Skin of the Caterpillar, as it is about the Superficies of all other Bodies: He has endeavoured to contrive it ſo, as that no Air might remain thus ſticking to the Skin of thoſe Inſects upon which he has made

these Experiments. And for this Purpose, before he plunged them in the Water, he first washed them all over with a Hair-Pencil or Brush; and these being afterwards immersed in the Water, but very few Bubbles of Air have been discovered on the Outside of their Bodies; and fewer as it appeared than Mr. *de Reaumur* had found upon those, upon which he made his Experiments; neither was this last of Opinion that all those Bubbles which he took notice of were formed by the Air rushing out through the Pores, but that some of them were also formed by the Air sticking about the exterior Part of the Skin.

When a Caterpillar is plunged in Water, one Bubble of Air is almost constantly observed upon each of the *Stigmata*. Mr. *de Reaumur* concluded, that the Air was not expired by these *Stigmata*, because he could never observe that any Bubbles of Air were ever driven out of these *Stigmata*, as one would think there must have been, if the Air was really expired by these Apertures. Mr. *Bonnet*, on the contrary, has seen some Bubbles of Air come out from these *Stigmata*, and that has contributed to make him rather think that the Air inspired was also discharged at these same Orifices. But as these Experiments are not decisive, he is unwilling absolutely to determine, but proposes the making more new Experiments.

A Caterpillar can remain several Hours under Water without perishing; it only falls into a State of Numbness; but if again taken out of the Water, it is not long before it again shews Signs of Life, and recovers. Mr. *Bonnet* has sought by some

Experiments,

Experiments, to know, if some only of these 18 *Stigmata* of a Caterpillar might not be sufficient for the Purposes of Respiration: He has plunged some of them only partially in Water, sometimes by the Tail, and others by the Head foremost; but always so that either 2 or more *Stigmata* might be out of the Water; and in these Cases the Caterpillar has not fallen into the torpid State above-mentioned, as it constantly did when intirely immersed. He has lifted out of the Water some of the *Stigmata* of Caterpillars that had been quite immersed, and that were so become torpid and motionless; and these have also soon after shewn Signs of Life and Motion. One of the Caterpillars, upon which Mr. *Bonnet* made Experiments, lived 8 Days, suspended in the Water, and only exposing to the Air its posterior *Stigmata*; that is to say, that only the 2 last *Stigmata* were out of the Water.

He during this time carefully observed his Caterpillar; and he remarked, from time to time, when the Insect moved itself, that little Streams of Bubbles came out of the anterior *Stigma* on the left Side. It appeared to him, by this and some other Experiments, that amongst all the 18 *Stigmata*, the two anterior and the two posterior ones are of a greater Use for the Respiration of the Caterpillars than any of the others. He also found, that, upon the choaking up these *Stigmata* with Butter, the Animal seemed to suffer much more sensibly, than when he so choaked up all the intermediate ones.

All these Experiments of Mr. *Bonnet*, and which are very particularly detailed in his Paper, were made

with great Attention, Patience, and Sagacity. And it is to be wished that he may continue thus diligently to apply himself to the Study of Natural History.

VI. *Divers Means for preserving from Corruption dead Birds, intended to be sent to remote Countries, so that they may arrive there in a good Condition. Some of the same Means may be employed for preserving Quadrapeds, Reptiles, Fishes, and Insects, by M. de Reaumur, F. R. S. and Memb. Royal. Acad. Sc. Paris. translated from the French by Phil. Hen. Zollman, Esq; F.R.S.*

Read from March 10.
1747-8. to April 27,
1748.

PERSONS who have at Heart the Progress of Natural History, and intend to facilitate the Study of it, must needs be desirous to see the Collections of divers Sorts of Productions, which form the Objects of it, multiplied and enlarged, and therefore will be disposed to contribute towards it with all their Ability. Those Collections present together in one Place more different Sorts of Bodies of the Mineral, Vegetable, and Animal Kingdoms, there to be at Leisure compared and examined one against the other, than one could hope to find successively in the longest and most laborious Voyages and Travels. In order to render those Collections

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as complete as possible, there should be in all the Countries of the World Men zealous for their Improvement, who should take Pleasure in transmitting the particular Productions of those Parts which they inhabit, to such Repositories as they know to be already considerable, and intended to be rendered useful to the Public.

That Part of Natural History which can offer to us the largest Series of agreeable Objects, and actually offers a vast Number which are not sought after merely for the Pleasure of looking upon them; *viz.* that Part which treats of Birds, has remained as yet very imperfect, nor has it yet made them sufficiently known to us, because no considerable Collections have hitherto been made of them; and those who had begun to make any soon became weary of going on, having had the Mortification to see them every Day destroyed by ravenous Insects, in spite of all the Care that had been taken to preserve them against their Teeth. *M. Reaumur* having found easy Methods of preparing Birds which are intended for those Collections, so as to put them out of Danger of being spoiled, and to make them look as if alive, has thus found what was still most desired, *viz.* the means of putting them out of Harm's Way from greedy Insects. He intends soon to inform the Public how to render with Success this sort of Collections durable. He has had the Luck to make one, which is already very numerous, and has Room to hope that it will be still larger. The Birds, for which he is obliged to several learned Men, Lovers of Natural History, are an Earnest to him that he shall owe Thanks to them

them for more, according as they shall find Opportunities to procure them for him: Besides he is sensible how much he may depend on their good Disposition to instruct him, for which he is very thankful.

However desirous one may be of sending Birds of the Country where one lives, to another, where the like are not to be seen, one may be at a Loss how to send them on a long Journey without their being disfigured or falling to Pieces by Corruption on the Way. I am going to explain here the different Means one may have recourse to, for keeping them from Corruption, and to make them arrive in a good Condition.

The first Way.

The Method hitherto practised to acquaint Natural Philosophers of very remote Countries with Birds of another Country, is to send them stuffed, that is to say, to take off their Skin with all the Feathers upon it, from the Body and the Thighs, leaving the Legs, the Wings, and for the better Conveniency the whole Neck with the Bill sticking to it. Filling afterwards the Skin thus taken off with some soft Stuff, either Straw, Hay, Wool, or Flax, &c. or even stretching it over a solid Mould of the Shape of the Bird, you give to this Skin, as near as possible, the Form of the Body of the Bird, which it had when it covered its Flesh and Bones; in which one sometimes succeeds tolerably well, by Attention, and some small Processes, the Particulars of which are not intended here to be entered into.

The

The second Way.

The foregoing Way of preserving the Shape of Birds requires a Hand used to it, and even falls short of sufficiently imitating Nature, unless with Care and Time. So it is certainly most convenient only to send the Bird as it has been received. There is no great Skill required for putting one or several into a Vessel full of Spirit of Wine, or very strong Brandy. It has been usual for a long time to make use of those Liquors with Success for preserving the Flesh of dead Animals; and wherefore has this Method so seldom been used hitherto to prevent whole Birds from Corruption? Perhaps it is because their Feathers do not shew those various and bright Colours, which are natural to them, whilst they are immersed in some Liquor, and which appear no longer on the Bird's Feathers when taken out of it. Besides, the Vanes of the Feathers are then disordered, and glewed too much together. Upon these first Appearances, it was judged too hastily, that spirituous Liquors changed the Colours of the Feathers, and hinder'd the reducing of them to the Order and Pliability they had upon the Animal, when dry and living. However repeated Experiments have made M. *Reaumur* sensible, that the Colour of the Feathers is Proof against the strongest Brandy, and even Spirit of Wine, and that after having dried the Bird that had been soaked, one may easily put its Feathers into their natural Order, and make it appear as it was when alive:

1. To preserve Birds which are to be sent far off, you are only to keep them in Brandy; the stronger it is the better it will be for producing the intended Effect: Spirit of Wine is even preferable. As for the rest, it is indifferent whether the Brandy be distilled from Wine, Corn, or Sugar.

2. Though the Birds may be put into the Liquor so as one receives them, yet some small Attention is to be had, and some Precautions to be used, before they are dipped in, which contribute towards preserving them in a more perfect State. If any of the Bird's Feathers are bloody, you must wash them from time to time with a wet Linnen, till they do not any longer leave a Mark upon that Linen, or in the Water in which they are soaked. Above all it is of Consequence to hinder the Feathers from taking a wrong Bent, or rumpling. It is easy to put them into the Shape they are to be, by smoothing them with a Finger from the Head towards the Tail in squeezing them together. This helps the Feathers to take the Position which is most natural to them, and in this Position they are kept by wrapping the Bird up in a Rag, tying about the Neck and the Body several times a strong Packthread: The Feathers on the Neck are chiefly those which must be kept from turning aside or backwards.

3. The Precaution of taking out of the Body the Intestines and other Parts it contains, is not absolutely necessary; it is better however to do it: If afterwards one supplies their Place, by filling the Cavity of the Belly with all the Quantity it can contain of Wool, Hemp, Cotton, or other soft Matter; if you fill the Neck, though without distending

been weakened by Evaporation, and by the aqueous Juices extracted from the Flesh.

6. If those Birds are not to arrive by Sea to their Journey's End, if they are to be cartied by Land for Part of their Way, one must contrive it so, that they may not be liable to be tossed by much Jumbling; and they will be less so, if the Vessel is so much fuller of them; they will close the more together. In case they should float too much in the Liquor, you need not scruple to press them with Hay or some other Stuff, which you thrust into the Vessel.

7. It is still more easy to hinder the Birds from being tossed, and they will even be the better preserved, if before you send them you take them out of the Liquor, in which they have lain a sufficient Time; it has made them fit to dry without any Danger of Corruption. Small Birds, such as of the Bigness of Sparrows, and even of Black-birds, after having been cover'd 8 or 10 Days with strong Brandy, may be taken out without any Fear of their being corrupted. Large Birds, and especially such as are very fleshy, are to be kept longer in the Liquor; but there are none or few, for which it may not be enough to have lain in it a Month or five to six Weeks. According as you take them out, you must range them one next to the other, and upon one another in a Box, filling up the Intervals with a Matter easiest to be had, as Chaff of Oats or Barley; that is to say, those small Shells in which the Grain was wrapped up whilst it stuck to the Ear. This Chaff is the best Stuff for this Use: You may also use small Hay, Moss, Hemp, Cotton, &c. Far from its being necessary to leave the
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the Birds to dry before ranging them in the Box, the best is to put them in quite dripping with the Liquor. Having filled the Box well, there remains only to shut it up.

8. Any Box, of what Form soever, may be fit for Birds which are to be on the Journey only for some Weeks or a few Months: Such as are to travel Years, require more Precaution; though they are not subject to Corruption, yet they may be torn to Pieces before their Arrival, if Insects greedy of them can come at them, and multiply in their new Habitation. One may by Care so well close up those Boxes, as to render it impossible for those dreadful Insects to get to the Inside; Paper glued over all the Joints will prevent it. But Barrels are preferable to Boxes, for such Birds as are to remain shut up for a Year or longer; the smallest Insects will not find a Passage for creeping into a Barrel, which will not permit the smallest Drop of Liquor to get out. Birds being put wet into the Barrel, keep from drying up too much, and keep one another the closer. As good Luck will have it, carnivorous Insects are none of those that will pierce Wood. So by using Spirit of Wine or strong Brandy, as we just now said, one will succeed in having those Birds arrive in a good Condition at the remotest Places. There is still another Way for it, which may appear more convenient, especially for Birds of a large Size.

The third Way

Is to preserve Birds by a sort of Embalming, and even by actual Embalming, in Countries
Sf 2
where

where the Spices are cheap. First, you begin with emptying the Body of the Bird, and then fill it with those Powders I am going to specify to you; you also fill its Neck with the same Powder, thrusting it in through the Bill. If the Bird is extremely fleshy, you may make an Incision in the Flesh of the thick Part of each Leg, and one in the Flesh of each Wing; that is to say, two on the Breast, and one nearer the first and large Bone of each Wing, into which you put the Powder; having afterwards brought the Flesh together again, and put the Feathers in Order, those Incisions will be hid so as not at all to disfigure the Bird. But there are very few on which it was necessary to make such Incisions; one may make some even inwardly, which will serve as well; having thrust your Fingers into the Belly, you may tear the Integuments over-against the thick Part of the Leg, and in other Places, and make Cavities to be afterwards filled up with the Powder.

2. There are many Powders proper to produce the principal Effect intended here, which is to promote the Bird's drying before it be so far corrupted as to occasion the falling off of the Feathers. All sorts of Spices may be used for it with Success; if there are any in the Country which are very cheap, you may use them. You may even make use of a Powder composed of as many Sorts of Spices as you will, the Result of which will be at least, that the Bird, after being dried, will smell the sweeter, and become as it were a Piece of Perfume. But instead of using resinous Gums, as Aloe, Myrrh, Frankincense, and other Productions of Plants, as Cinnamon, Cloves, Pepper, Ginger, &c. which are dear Materials,

terials, you may content yourself with a Salt which is cheap in most Countries; it is sufficient to fill the Cavity of the Body and of the Neck with Alum reduced to Powder. A Material still easier to be had in all Places, and very cheap, and which works with great Effect, is Lime. If it can be had quite unslack'd, you will take it preferably; however, without scrupling to take such as is old, and which has been somewhat slackened by the Humidity of the Air.

After the Body and the Neck of the Bird have been filled up, either with pulverized Lime, Alum, or any other Powder, you put it into the Box or the Barrel, in which it is to be transported. You will take care, in placing it, to give a natural Position to the Neck, neither to give to the Legs any other Inflexion than they had when the Bird stood upon them alive. At the Bottom of the Box or the Barrel there is to be a Layer of the Thickness of an Inch, or thereabouts (if there be more there will be no Harm) of the same Powder with which the Cavity of the Body is filled, or of any of those which are proper for it. You bury the Bird in this Powder, and put enough of it about it and upon it, so as to cover it with a Layer of the Thickness of an Inch or more. The outward Powder will make it dry the sooner, and keep off voracious Insects, which will not care to attempt to pierce through it in order to come to the Flesh they are fond of. During the first Days, and even during the first Weeks, the Birds may cast a bad Smell, which you need not be uneasy at, for it will lessen in proportion to the Bird's drying; and it will dry

so that none of the Feathers will come off; and when it is once dried, they stick fast to it for ever. This Way of preserving Birds, which is very simple, has procured to M. *Reaumur* some from very remote Countries, which arrived as wished for.

The fourth Way.

This is one, by which Birds are more speedily dried, than by that which is explained before; it is to dry them by the Heat of an Oven. You make use of that Heat which remains in it after the Bread is taken out of it; sometimes it is then too great, but there is a plain Way to be sure that the Degree of Heat is not too great, which is, to put Feathers into the Oven, and to take them out 5 or 6 Minutes after; if you find that they are not singed, nor turned red, you ought not to be under any Apprehension for the Feathers of the Bird, which is to be put into the Oven. Small ones need remain in it only one or two Hours to be sufficiently dried; those of a middling Size require a longer Time; and those which are big, and very fleshy, ought to be put in at several times. When they are grown cold, you may know whether they are dried enough, by pressing with the Finger the Flesh of the Legs and of the Breast; if it does not yield, or yields but little under the Finger, the Bird does not any more want to be put into the Oven. The Inconveniency attending its being kept there longer than is necessary, is, that some Parts of it, as for instance, the Neck and the Rump, are thereby render'd too brittle. You will prevent the Bird's Bulk sensibly diminishing

minishing in the Oven, if, before you put it in, you fill the Cavities of its Body and the Neck with some soft Stuff, like any of those which we mention'd to be us'd for filling the Cavities of such Birds as are intended to be preserv'd by the means of Spirit of Wine, *viz.* Hemp, Flax, Cotton, &c. What is the most difficult in the Way of drying Birds in the Oven, is not hitting the proper Degree of Heat, and to know the Time how long they are to be kept in it: Here will be the Difficulty, how, as this Way of drying requires the Bird may be kept in a natural Attitude, before it is put into the Oven: If dried, it will be fixed for ever in that which it once received. There are several Ways, plain in themselves, for putting and keeping the Bird in its natural Attitude, which however would be too long to be explain'd as to the Particulars; the little we shall say of them, will be sufficient to industrious Persons for their Use. The Bird may be kept in Order by the means of a Frame, made like a Farrier's Travise; it is composed of a small Board, which forms the Basis of it, the Length of which need not be greater than that of the Bird: On each Side of this Board rises an upright Post of Wood; these four Posts are secured by Traverses fixed to them by small Nails: The Use of those Posts and Traverses is to keep fixed the small Ribbons and Threads, which keep the Body, the Wings, and the Neck of the Bird in the Position it has been brought to. A Thread run through the Head of the Bird, with the Help of a Needle, enables you to place it as high or low as you please. There are various Ways of fixing the Feet on the Board, with the Claws extended;

extended; it may be done with small Points of Nails. With a Wire only, and a small Board, all may be done as well as with a Frame: This Wire is run through all the Length of the Body and of the Neck of the Bird, by insinuating it through the *Anus*; but before doing so, you make a sort of a strong Knot to it, by twisting it; this Knot is to touch the *Anus*; it afterwards hinders the Bird from sliding: Close by the Knot you bend down perpendicularly that Part of the Wire which is without the Body, and which is to be at least of a Length equal to the Height which the Legs are to have; you make afterwards its End pointed by filing, if you have not already done it, and you run it into the Board. That Part of the Wire which then is out of the Body, serves for a Supporter, which keeps the Bird raised, because it is continued to the rest of the Wire which runs through the Body and the Neck: The Wire which runs through the latter keeps it in its bending Way, and the Direction that has been given to it.

Dried Birds ought to be sent in Boxes or Barrels sufficiently closed up, that Insects may not slip in during the Journey; and you will take care to fill up all the empty Spaces left in the Barrel with some of those soft Stuffs, which we have already pointed out for such Uses. Many Weeks, nay even Months, may pass between the Time, when you have dried the first Birds you intend to make a Collection of for a Journey, and that Time when they are to set out: This Interval is dangerous. There are certain Worms, and certain Beetles, which are more greedy after those dried in the Oven, than after those dried
any

any other Way; if they meet with free Access, they sometimes seize the first Moments to settle under the Feathers, or in the Bodies, where they multiply.

You will put your Birds out of the Reach of the formidable Teeth of those Insects, if after they have been taken out of the Oven, you bury them in Sand contained in a large Box or a Barrel. You must take care in covering them with Sand, that they may not contract bad Attitudes, and that their Feathers be not ruffled. Slack'd Lime reduced to Powder, Chalk, and all earthy Powders, fine and dried, may be successfully employ'd for the same Use. You will press with your Hand the Surface of the Powder, to render the uppermost Lay compact, which is very necessary. Lastly, if from the falling of the Feathers it appears that the Insects have defeated the Precautions taken against them, there is still a Remedy left; you may stop the Progress of the Evil by putting the Bird again into the Oven, not hot enough to singe the Feathers, but hot enough to kill the Insects in less than half an Hour.

Remarks that are common to the four Ways of preparing Birds.

1. It will not be amiss to send two or three Birds of each sort; and, as near as you can, let there be one Male and one Female.
2. One cannot help being curious to know the Name which each Bird bears in the Country where it was taken: You write it with common Ink upon a Slip of

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Parchment,

Parchment, which you tie with a Thread to one of its Feet; the Writing will be preserved, even when the Bird is in Brandy.

3. If you know of a Bird any thing besides its Name, you may make a small Note, shewing in what Places it lives; what it feeds upon; whether or no it stays all the Year in the same Country; how and where it makes its Nest; how many Eggs it lays; the Wiles and Cunning particular to it; whether it is good to eat; in short, whatever is known of its History.

4. A Collection of Nests is a proper Repository to be joined to that of Birds; it shews such Works as hardly could be imitated by Men, admirable for their Form, their Workmanship, and the Materials employed in them: M. *Reaumur* has already made such a Repository. If one can have Nests not too bulky for easy Transportation, you may be sure to see them with Pleasure joined to the Birds that have built them.

5. The Colours and Figures of the Eggs make also Part of the History of Birds; Collections made of them will give Satisfaction to curious Minds: Those which are to be sent would be in Danger of being broken on the Way, by the very Substance they contain, if it comes to ferment. Before you send them therefore, you must empty them: To this end you make a small Hole on each End, and shake them; and if this Shaking will not be enough, you blow into one of the Holes to force out through the other what liquid Matter remains in the Egg.

Quadrupeds.

Quadrupeds that are not of too large a Size, and particular to certain Countries, may be put into a State fit to be sent to the most remote Parts, by one of the four Ways used to preserve Birds: You may make durable Collections of them like those of the latter. M. *Reaumur* has begun one, which makes Persons who see it wish that there might be more complete ones of the same kind.

Fishes and Reptiles.

Fishes and Reptiles, which, as well as Quadrupeds, are engaging Objects for Naturalists, are easier to be sent; it is sufficient to put them into Barrels full of strong Brandy. They may also be dried, either by Materials with which you may fill the Cavities of their Bodies, or by a gentle and well-manag'd Heat.

Insects.

Insects, which offer to us so many admirable Varieties, deserve the Care of gathering them into Collections, which cannot but be precious to those who have made those little Animals their Study. All those which are soft, as for instance Worms and Caterpillars, may be preserved in Brandy. Their tender Colours will run less Hazard of being alter'd, if you put into the Brandy such a Quantity of Sugar as it is able to dissolve. Beetles may also be put into the same Liquor; but Butterflies and Flies would be spoiled in it: After having killed them, you must range them in Lays in Boxes, and separate those Lays with Beds of Cotton. Though one ought to collect in each Country, preferably, those which strike

most, either by the Variety and Lustre of their fine Colours, or by their Size, or by their singular and odd Form, or by the Use one knows to make of of them; yet you ought not to neglect to collect and send such as do not offer so remarkable Singularities, such as even are most common. There are among the latter some, which have wherewithal to satisfy an Observer, who looks upon them with other Eyes than those wherewith they had been regarded before, and with other Views.

VII. *A beautiful Nautilites, shewn to the Royal Society by the Rev. Charles Lyttleton LL.D. F. R. S. and Archdeacon of Exeter.*

Shewn May 5. 1748. **T**HIS curious Fossil seems to be composed of a stony Matter like Marble, which has penetrated the Cells of the *Nautilus* while in its natural State. The Diaphragms or Partitions remain still distinct and visible. The different Colour of the stony Matter in some Cells of a dark-brown or Hair-Colour, in others of a light-brown or Ash-Colour, with the natural Polish of the Outside, gives it a beautiful Appearance; as it is represented in the annexed Print (*See TAB. Fig. 2.*) where it is drawn of its natural Size in three different Views.

A shews the Side View of it.

B the fore Part.

C the back Part.

C. M.

It

It was found in *Pool's Hole* in *Derbyshire*. Its Sutures or Diaphragms resemble those of some of the larger *Cornua Ammonis*; but its Shape bespeaks it to be a Species of *Nautilus*; and it is thought to be a Non-descript, both in its natural and fossil State.

VIII. *The Substance of a Letter from Mr. William Arderon F. R. S. to Mr. Henry Baker F. R. S.*

Read May 12.
1748.

OF all the several Kinds of Fish which for some Years past I have been keeping in glass Jars (in hopes of becoming acquainted with the Nature and Properties of these Animals, by having them daily under my Inspection) none seems more impatient of Imprisonment, if I may so call it, than the Roach; nor, if they are well look'd after, and supplied often enough with fresh Water, have I observed any, except the Roach, to become distempered. But most commonly, after this Fish has been a little while confined, the finny Part of its Tail begins to drop off Piece by Piece; and when the finny Part is all gone, a sort of Mortification seizes upon the Tail itself, and gradually creeps along until it reaches the Intestines, at which time the Fish immediately dies.

The last Roach I had under this Disorder was about the Beginning of *January*; when in the Space of a Month, it had lost the greatest Part of the Fin, which induced me to clip off the rest, hoping thereby

thereby to stop the Progress of the Mortification. But this was of no manner of Service that I could perceive: The Distemper still gained Ground; and as it increased, a fine fibrillous Substance grew out from it, and appeared like what the Picture shews at *Fig. 3.* in TAB.

These Fibrils, when examined by the Microscope, shew themselves to be a Number of minute Tubes, filled with a brownish Liquor; and this Liquor, upon pressing them, becomes immediately discharged.

A small Piece of this Fish, with the Fibrils growing out of it, as seen by the fourth Magnifier of Mr. Cuff's double Microscope, is shewn at *Fig. 4.*

When first I perceived this fibrous Substance enveloping the Fish's Tail, I supposed it to be nothing but a Mouldiness, of that kind which frequently is seen upon decayed Flesh and Fish; but, upon Trial, I found it to be of a much stronger Texture and Consistence than such Mouldiness is ever known to have; for, notwithstanding I have several times let a full Stream of Water run upon it from a Cock, I could never wash it off.

This Fish lived with me till the latter End of *March*, and then died; having for many Days before its Death lain at the Bottom of the Jar, without being able to rise.

As the Mortification advanced, and came nearer to its Intestines, the Quickness of its taking Water in at the Mouth increased, till at last it took it in three times faster than a lively strong Fish did.

On my cutting off Part of the Fish's Tail, in hopes of stopping the Mortification, the Equilibrium of the Body was so far lost, that it hung in the Water most commonly with the Head downwards,

and could never afterwards continue in any other Posture, without great Strugglings, or sinking down to the Bottom of the Vessel. Which may serve to shew how nicely and wonderfully the Bodies of Fishes are balanced, for the keeping them in an horizontal Position; since in this Case the losing a few Grains of the Tail could so sensibly destroy the Equilibrium, as to render the rest of its Fins almost useless.

I dare not however assert it will happen thus to all sorts of Fish on cutting off the Tail; nor does it to the Roach immediately: For as it is a Posture very unnatural and troublesome to Fishes, they exert all their Strength to prevent their Heads from sinking downwards; until, being wearied out, they at last are forced to submit. I remain,

Dear Sir,

Norwich, April 14.

1748.

Your most obedient Servant,

W. Arderon.

IX. *A Letter from Mr. Robert Roche to the President, of a Fustian Frock being set on Fire by Electricity.*

Honoured Sir,

London, May 17, 1749.

Read May 19.
1747.

IF your Goodness will excuse the Liberty a Stranger has taken in giving you this Trouble, hoping the following Account will

will atone for his Boldness, I shall think myself greatly favoured.

I have a Son about 16 Years old, that has been for 6 or 7 Years past troubled with sudden Fits that intirely take away his Senses. I got him all the Helps I could, but to no purpose; at last I sent him to *St. Bartholomew's Hospital*, as an Out-Patient; and there he was turn'd out as incurable. So finding his Case desperate, I considered the Power of Electricity, and made a large Machine for Electrifying; and afterwards shocking him commonly twice a Day, he has received some Benefit: And last *Sunday*, being *May 15*, he being on the Pedestal, and very high electrified, and having on a coarse Fustian working Frock, the condensing Phial being on the Conductor, and I, touching him to procure Snaps as usual, touched his right Shoulder Blade; and, to my great Surprise, the furzy Flax of the Frock caught Fire, with a great Blaze, and burnt the whole Breadth and Length of the Shoulder, the Flame rising 6 Inches above the Collar, and I believe would have set the Frock on Fire, had I not put it out with my Hands. There was no Fire in the Room that Day: This was about Noon; neither was there any thing that could have any inflammable Vapour there.

My Surprise was the greater, because all I read on that Subject says nothing will burn but what sends forth such Vapours.

At 9 the same Evening I made him put on the same Frock, and touch'd the left Arm, where the Flax had not been burnt before; and it had the same Effect as above.

Sir,

Sir, if any further Account of the above will be acceptable to you, or the *Royal Society*, if you please to command I will wait on you. There are Alterations in my Machine I think for the better; and some new Experiments too long to write, fearing it would be too troublesome; from

Your humble and obedient Servant;

R. Roche.

X. *A Letter from John Huxham M. D. F. R. S. to C. Mortimer Secr. R. S. concerning a Child born with an extraordinary Tumour near the Anus, containing some Rudiments of an Embryo in it.*

Dear Sir,

Read May 19. 1748. **T**HE following Case was lately communicated to me by Mr. *Alexander Wills*, an experienced and ingenious Surgeon and Man-Midwife, of *Kingsbridge*. It seems to have something particular and remarkable. If you think so (on Perusal) you may be so good as to lay it before the *Royal Society*. I am,

S I R,

Plimouth, Dec. 12.
1746.

*Your much obliged, and
most obedient humble Servant,*

J. Huxham.

U u

“ John

“ *John Perrine’s Wife*, of *Charleton* Parish, in
 “ this County, a brisk active young Woman (he
 “ very infirm and consumptive) was deliver’d of a
 “ Daughter at full Time, *July 11, 1746*. The
 “ Child was perfect, as to all its Limbs, Head,
 “ Body, &c. but from the Region over the *Os*
 “ *sacrum, Glutæi* Muscles, and between the Thighs,
 “ quite home to the *Pudendum*, was growing a
 “ very large Substance, which the Midwife and
 “ others call’d a Wen, in Shape very like the Ven-
 “ tricle of a Sheep, and seem’d, as to its Colour
 “ and outward Appearance, a Continuation of the
 “ same Skin with the rest of the Body, but very
 “ full of Blood-vessels. It hung down behind be-
 “ low the Heels, and was bigger than the whole
 “ Body of the Child itself. It felt very soft, and
 “ seem’d to have Matter fluctuate in it; but in the
 “ middle of the Whole was evidently felt a hard
 “ Substance.

“ The *Pudendum* as well as *Anus* were in all re-
 “ spects natural, and both Urine and Stool were re-
 “ gularly discharg’d; but the *Anus* was placed much
 “ more forward, and immediately under the *Pu-*
 “ *dendum*; so that the *Fæces* were discharged in
 “ the same Direction with that of the Urine.

“ I made a Puncture in the depending Part of
 “ the Tumour, and drew off near two Quarts of a
 “ palish red Water, without any Smell. The Ori-
 “ fice being left open, there was a continual Issue
 “ of the same kind of Water for several Days; but
 “ by degrees it became more and more glutinous,
 “ and at length whitish like *Pus*, and very fetid. As
 “ the

“ the Discharge was great, the Child grew weaker
 “ and weaker, and at the End of 15 Days died.

“ The next Day I open'd the Tumour, and found,
 “ near the *Os Coccygis*, an Abscess within a Cystis,
 “ in which were four Ounces at least of white
 “ Pus prodigiously stinking; and, on further Exa-
 “ mination, found several cartilaginous Joints, as it
 “ were, somewhat resembling the Tail of a Sheep,
 “ continued from the Point of the *Os Coccygis*.
 “ These were about two Inches long, and envelop'd
 “ with a kind of fleshy Substance cover'd with a
 “ sort of Fat: These, when cut thro', appear'd ex-
 “ actly like the inner Part of Lamb-Stones. From
 “ those depended a Substance like the Head and Neck
 “ of an Embryo, as big as a large Egg, which, on
 “ opening, contained somewhat resembling Brain,
 “ and a kind of a *Cerebellum* in the back Part:
 “ It had a Mouth and Tongue on one Side of the
 “ Face (if it might be so call'd) but no Appear-
 “ ance of Eyes or Nose; however there was an
 “ Ear pretty evident.

“ In the large Tumour there hung a kind of
 “ loose Membrane, which perhaps might be Part
 “ of a Secundine.

XI. *Of the Fluents of Multinomials, and Series affected by radical Signs, which do not begin to converge till after the second Term; in a Letter from T. Simpson F. R. S. to W. Jones Esq; V. P. R. S.*

Presented May 26.
1748.

ALTHO' the Application of infinite Series, and the Quadrature of the conic Sections, to the inverse Method of Fluxions has exercised the Pens of the most able Mathematicians, and produced many curious and useful Discoveries, yet nothing has been hitherto given, that I know of, whereby the Fluents of radical Multinomials and Series, which do not begin to converge till after the second Term, can be determined, so as to be of Use in the Solution of Problems: The common Method, by expanding the given Expression, being, you know, altogether impracticable in this Case.

The Consideration of which induced me to draw up the following Paper; which I humbly beg Leave to lay before you, who are so good a Judge of the various Improvements which this Subject has from time to time received.

What most encourages me to hope this little Essay will meet with your Approbation, is, that it is not merely an abstracted useless Speculation, but may be apply'd to good purpose in many difficult and important Enquiries into Nature; whereof I have put down one or two Instances, and shall further take the Liberty to observe here, that most of the
lunar

lunar Equations, given by Sir *Isaac Newton*, are only such Approximations as may be exhibited by the first Term of a Series derived by the Method here delivered.

Proposition.

The Fluent of $a + cz^n$ $\times z^{pn-1} \dot{z}$ being given (either in algebraic Terms, or from the Quadrature of the Conic Sections, &c.) it is proposed, by means thereof, to approximate the Fluent of $a + cx^n + dx^{2n} + ex^{3n} + fx^{4n}$ &c. $\times z^{pn-1} \dot{z}$; supposing the Series not to converge till after the second Term.

Make $cz^n = cx^n + dx^{2n} + ex^{3n}$ &c. and let Q be the given Fluent of $a + cz^n$ $\times z^{pn-1} \dot{z}$, answering to any proposed Value of x : Moreover let $y = x^{pn}$, or $y^{\frac{1}{p}} = x^n$, and let this Value of x^n be substituted in the first Equation, and it will become $cz^n = cy^{\frac{1}{p}} + dy^{\frac{2}{p}} + ey^{\frac{3}{p}}$ &c. whereof the Root y being extracted, we shall (by making $R = -\frac{pd}{c}$, $S = \frac{p \cdot p + 3}{2} \times \frac{d^2}{c^2} - \frac{pe}{c}$, $T = \frac{-p \cdot p + 4}{6} \times \frac{d^3}{c^3} + \frac{p \cdot p + 4}{1} \times \frac{de}{c^2} - \frac{pf}{c}$ &c.) have $y(x^{pn}) = z^{pn} + Rz^{pn+n} + Sz^{pn+2n}$ &c. whence we also obtain $x^{pn-1} \dot{x} = z^{pn-1} \dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1} \dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1} \dot{z}$ &c.

Let this Value, with that of $cx^n + dx^{2n} + ex^{3n}$ &c. (above given) be now substituted in the proposed

posed Fluxion, and it will become $\overline{a + cz^n}^m$
 $\times z^{pn-1} z^{\frac{p+1}{p}} \times R z^{pn+n-1} z^{\frac{p+2}{p}} \times S z^{pn+2n-1} z^{\frac{p+3}{p}} \&c.$

Moreover, let v denote the Place, or Distance, of any Term, of this Expression, from the first (exclusive) then the Term itself (drawn into the common Multiplicator) will be denoted by $\overline{a + cz^n}^m$
 $\times \frac{p+v}{p} \times A z^{pn+vn-1} z$; and the Fluent thereof will

be truly expressed by $\frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \dots$
 $\frac{p+v}{p+m+v} \times \frac{a}{-1} \times A Q + \frac{p+v.A}{p} \times \frac{\overline{a + cz^n}^{m+1} \times z^{pn-n}}{p+m+v \times zc}$ into

$z^{vn} - \frac{p+v-1}{p+m+v-1} \times \frac{az^{vn-1}}{c} + \frac{p+v-1.p+v-2}{p+u+m-1.p+v+m-2} \times$
 $\frac{a^2 z^{vn-2n}}{c^2} \&c.$ continued to as many Terms as there

are Units in v . Wherein let v be expounded by 1, 2, 3 &c. successively, and $R, S, T, \&c.$ by A respectively: By which means the Fluent of the whole Expression will be obtained.

Corol. 1.

Because the Fluent of the general Term, when the Multiplicator $\overline{a + cz^n}^{m+1}$ becomes $= 0$, is barely
 $= \frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \dots \times \frac{p+v}{p+m+v} \times \frac{a}{-1} \times A Q$
 the Fluent of the whole Expression will, therefore, in this Case be truly defined by

$$\mathcal{Q} \times 1 = \frac{p+1. Ra}{p+m+1. C} + \frac{p+1. p+2. Sa^2}{p+m+1. p+m+2. C^2} + \frac{p+1. p+2. p+3. Ta^3}{p+m+1. p+m+2. p+m+3. C^3} \&c. \text{ Where } \mathcal{Q} \text{ denotes}$$

the Fluent of $a + \sqrt{c z^n}^m \times z^{pn-1} \dot{z}$, when $z^n = \frac{a}{c}$.

Corol. 2.

But, if $m+1$ and p be, each of them, the Half of an odd affirmative Number, and P be taken to denote the Periphery of a Circle whose Diameter is Unity, and $-c$ be put $=b$, then the Value of \mathcal{Q} (or the Fluent of $a - b z^n^m \times z^{pn-1} \dot{z}$, when $z^n = \frac{a}{b}$)

will be $= \frac{a^{p+m}}{nb^p} \times$

$\frac{1. 3. 5. 7 \&c. \text{ (to } p-\frac{1}{2} \text{ Factors)} \times 1. 3. 5. 7 \&c. \text{ to } (m+\frac{1}{2} \text{ Factors)}}{2. 4. 6. 8. 10. 12 \&c. \text{ (to } p+m \text{ Factors)}}$

Therefore the *Whole*, required, Fluent, of $a - bx^n + dx^{2n} - ex^{3n} \&c. \sqrt[m]{} \times x^{pn-1} \dot{x}$ is, in this Case, equal to the Product of that Expression into the following Series, $1 + \frac{p+1. Ra}{p+m+1. b} + \frac{p+1. p+2. Sa^2}{p+m+1. p+m+2. b^2} \&c.$ Wherein R is to be taken $= \frac{p d}{b}$, $S = \frac{p. p+3}{2} \times \frac{d^2}{b^2} + \frac{p e}{b}$, $T = \frac{p. p+4. p+3}{6} \times \frac{d^3}{b^3} + \frac{p. p+4}{2} \times \frac{d e}{b^2} + \frac{p f}{b}$, $\&c.$ according to what is above specified.

The Use of what has been deliver'd above will, in some measure, appear from the Solution of the
two

two following Problems, which I shall subjoin as Examples thereof. The first is;

To find the Time of Oscillation in the Arch of a Cycloid, in a Medium resisting according to the duplicate Ratio of the Velocity.

Let A denote the whole Arch of the Semi-Cycloid, or the Length of the Pendulum, a the the Arch described in the whole Descent, and x any variable Part thereof described from the Beginning of the Descent; and let the Density of the Medium be, every-where, as $\frac{x}{b}$: Then the Fluxion of the Time will be found =

$$a - 1 + \frac{2a}{b} \times \frac{x}{2} - \frac{2x^2}{2.3b} + \frac{4x^3}{2.3.4b^2} - \frac{8x^4}{2.3.4.5b^3} \& c. \Big|^{-\frac{1}{2}} \times$$

$\frac{2A}{2A} \Big|^\frac{1}{2} \times x^{\frac{1}{2}} \dot{x} *$: which being compared with

$$a - bx^n + dx^{2n} + ex^{3n} \& c. \Big|^m \times x^{pn-1} \dot{x} \text{ (vide Corol. 2.)}$$

we shall, in this Case, have $n=1$, $m=-\frac{1}{2}$, $p=\frac{1}{2}$,

$$a=a, b=1 \times \frac{2a}{b} \times \frac{1}{2}, \frac{d}{b} = \frac{2}{3b}, \frac{e}{b} = -\frac{1}{3b^2}, \frac{f}{b} = \frac{2}{15b^3} \& c.$$

$$\text{Whence } R = \frac{1}{3b}, S = \frac{2}{9b^2}, T = \frac{8}{45b^3} \& c. \text{ Also}$$

$$\frac{a^{p+m} P}{nb^p} \times \frac{1.3.5.7(p-\frac{1}{2}) \times 1.3.5.7(m+\frac{1}{2})}{2.4.6.8.10.(p+m)} = \frac{P}{b^{\frac{1}{2}}}, \text{ and}$$

$$1 + \frac{p+1.Ra}{p+m+1.b} + \frac{p+1.p+2.Sa^2}{p+m+1.p+m+2.b^2} \& c. = 1 + \frac{a}{2bb} + \frac{5a^2}{12b^2b^2}$$

* The Investigation of this, and the Fluxion in the following Example, are both given in my Essays.

$$+\frac{5a^2}{12b^2b^2}+\frac{7a^3}{18b^3b^3} \text{ } \&O{c}.$$
 Whence we have $\frac{P}{b^{\frac{1}{2}}} \times 1 + \frac{a}{2bb} + \frac{5a^2}{nb^2b^2}, \text{ } \&O{c}.$ for the Time of one Vibration of the Pendulum; which, by substituting $1 + \frac{2a}{b} \times \frac{1}{2}$ for its Equal $b, \text{ } \&O{c}.$ becomes $PA^{\frac{1}{2}} \times 1 * + \frac{a^2}{6b^2} - \frac{2a^3}{9b^3} \text{ } \&O{c}.$ From which it appears, that the Effect of the Resistance on the Time of Vibration, in small Arches, is nearly in the duplicate Ratio of those Arches.

Sir *Isaac Newton* (from whom it is impossible to disagree without being under some Apprehensions of a Mistake) has, indeed, given a very different Solution to this Problem (in *Princip. Prop. 27. B. 2.*). But as the Conclusion here brought out exactly agrees with what I have elsewhere given, by a different Method, I have great Reason to believe I have no where fallen into an Error.

The second Example I shall give as an Illustration of the foregoing Method is,

To determine the Apside Angle (or the Angle of the two Apses at the Center) in an Orbit described by means of a centripetal Force, which varies according to any Power of the Distance.

In order to which, let the Velocity of the Body at the higher Apse be to that whereby it might describe a Circle at the same Distance from the Center, in the given Ratio of p to Unity; also let

X x

that

that Distance be denoted by Unity; and, supposing z to denote any other Distance, let the centripetal Force be universally expressed by z^n . Then the Fluxion of the Angle at the Center will be expressed by

$$-pz \sqrt{p^2 + \frac{2}{z+1} \times z^2 - p^2 - \frac{2z^{n+3}}{z+1}} \quad \text{Put } a = 1 - p^2, \quad v = \frac{z+3}{2} \text{ and } x = 1 - z^2, \text{ and it will become}$$

$$\frac{\frac{1}{2} \sqrt{1-a} \times x^v}{1-x \times \sqrt{ax + \frac{1-vx-1-x^2}{1-v}}} = \frac{1}{2} 1 - a^{\frac{1}{2}} \text{ into}$$

$$a - \frac{vx}{2} + \frac{v \cdot v-2}{2 \cdot 3} \times x^2 - \frac{v \cdot v-2 \cdot v-3}{2 \cdot 3 \cdot 4} \times x^3 \&c. \left]^{-\frac{1}{2}} \times$$

$$x^{-\frac{1}{2}} \dot{x} + x^{\frac{1}{2}} \dot{x} + x^{\frac{3}{2}} \dot{x} \&c.$$

Now, to find the Fluent of the first Term hereof (drawn into the general Multiplier) or

$$a - \frac{vx}{2} + \frac{v \cdot v-2}{2 \cdot 3} \times x^2 \&c. \left]^{-\frac{1}{2}} \times x^{-\frac{1}{2}} \dot{x}, \text{ we have (as before) } n=1, m=-\frac{v}{2}, p=\frac{1}{2}, b=\frac{v}{2}, \frac{d}{b}=\frac{v-2}{3},$$

$$\frac{e}{b}=\frac{v-2 \cdot v-3}{3 \cdot 4}, \&c. \text{ Also } R=\frac{v-2}{6}, S=\frac{v-2 \cdot 4v-5}{72};$$

and consequently the Fluent itself (when the Body arrives at the lower Apse) $= \frac{P}{\sqrt{\frac{1}{2}v}} x$

$$+ \frac{v-2}{2v} xa + \frac{5 \cdot v-2 \cdot 4v-5}{48v^2} xa^2 + \frac{7 \cdot v-2 \cdot 16v^2-37v+22}{6 \cdot 48v^3}$$

&c. After the same manner the Fluent of the second

the second Term will come out $= \frac{P}{\sqrt{\frac{1}{2}v}} \times$

$$\frac{a}{v} + \frac{5 \cdot v-2}{4v^2} \times a^2 + \frac{35 \cdot v-2 \cdot 2v-3}{48v^3} \times a^3 \text{ &c. that of the}$$

$$\text{third} = \frac{P}{\sqrt{\frac{1}{2}v}} \times \frac{3a^2}{2v^2} + \frac{35 \cdot v-2}{12v^3} \times a^3 \text{ &c. &c. &c.}$$

Whence, by collecting these several Fluents together, we have $\frac{P}{\sqrt{\frac{1}{2}v}} \times$

$$1 + \frac{1}{2}a + \frac{20v^2-5v+2}{48v^2} \times a^2 + \frac{112v^3-63v^2+42v-8}{6 \cdot 48v^3} \times a^3 \text{ &c.}$$

for the Fluent of the whole Expression: And this, drawn into $\frac{1}{2} \times -1 - \frac{a}{2} - \frac{a^2}{8} \text{ &c. } (= \frac{1}{2} \times 1 - a + \frac{1}{2}a^2)$ will be

$$= \frac{P}{\sqrt{2v}} \times 1 + \frac{v-2 \cdot 2v-1}{48} \times \frac{a^2}{v^2} + \frac{v-2 \cdot 2v-1}{72} \times \frac{a^3}{v^3} \text{ &c.}$$

$$= \frac{P}{\sqrt{v+3}} \times 1 + \frac{n-1 \cdot n+2}{24} \times \frac{a^2}{n+3} + \frac{n-1 \cdot n+2}{18} \times \frac{a^3}{n+3} \text{ &c.}$$

which, in Degrees, gives $\frac{180}{\sqrt{n+3}} \times$

$$1 + \frac{n-1 \cdot n+2}{24} \times \frac{a^2}{n+3} + \frac{n-1 \cdot n+2}{18} \times \frac{a^3}{n+3} \text{ &c. for}$$

the true Measure of the Angle required.

XII. *A Letter from Dr. John Lining to C. Mortimer M.D. Sec. R.S. concerning the Weather in South-Carolina; with Abstracts of the Tables of his Meteorological Observations in Charles-Town.*

S I R,

Read May 6.
1748:

THO' I have not the Pleasure of a personal Acquaintance with you, yet as you are one of the Secretaries to the *Royal Society*, I take the Liberty to send you some Tables and Observations deduced from a Diary of the Weather, which I have kept for some Years past in this Town, which you may communicate to the *Royal Society*, if you think they will be acceptable. As an Account of the Instruments which I have used, and their Situation, is already published in the *Philosophical Transactions**, I shall not trouble you with a Repetition of those Affairs.

The Vicissitudes of the Weather, with respect to Heat and Cold, are perhaps no-where greater than in *Carolina*; and our Summer's Heat is probably not inferior to that under most Places of the Equator; nor is our Winter's Cold much less at some times than that in *Britain*.

From near eight Years Observation, the greatest Increase of the Heat of the Air, which I have discover'd in 24 or 30 Hours, in Spring, Summer, Autumn, and Winter, was 19, 24, 13, and 16 Degrees

* N^o. 470, p. 497-8.

Degrees of *Fahrenheit's* Thermometer; and the greatest Decreases of Heat, in the same Space of Time, in those Seasons, were 35, 32, 27 and 25 Degrees respectively. It frequently happens, that one Day is ten or more Degrees warmer than the preceding Day; but the Decreases of Heat are always greater and more sudden than its Increases. On the 10th of *January* 1745. at 2 *p. m.* the Mercury in the Thermometer was at 70; next Morning it had sunk to the 26th Degree; and on the 12th Day in the Morning it was at 15, which was the greatest and most sudden Change I have seen.

In Summer, the Heat of the shaded Air, about 2 or 3 in the Afternoon, is frequently between 90 and 95 Degrees; and on the 14th, 15th, and 16th of *June* 1738, at 3 *p. m.* it was 98; a Heat equal to the greatest Heat of the human Body in Health. In Winter I never but once saw the Thermometer so low as 15: Therefore the Difference between the most intense Heat and Cold of the shaded Air, in this Province, is 83 Degrees; which is a much greater Range than could well have been expected in this Latitude; and taking the Mean between those Extremes, 56 should be the temperate Degree of Heat in this Province: But the Sum of the thermometrical Altitudes, divided by the Number of Observations which I made for some Years together, gives 66, which may therefore more justly be reckoned the temperate Heat in *Carolina*, which exceeds 48, the temperate Heat in *England*, more than that exceeds the freezing Point.

The mean Heat of the shaded Air, in Spring, Summer, Autumn, and Winter, taken from the mean nocturnal Heat, and from the mean Heat at 2 or 3 *p. m.* is 61, 78, 71, and 52 Degrees.

The mean Heat of the shaded Air at 2 or 3 *p. m.* in Spring, Summer, Autumn, and Winter, is 65, 82, 75, 55 Degrees; and the mean nocturnal Heat in these Seasons is 57, 74, 68, and 49 Degrees. Therefore our Winter's nocturnal Heat, at a Medium, coincides nearly with the temperate Heat in *England*.

The Thermometer, when suspended five Feet from the Ground, and exposed to the direct Rays of the Sun, and to those reflected from our sandy Streets, has frequently rose in a few Minutes, from 15 to 26 Degrees, above what was at that time the Heat of the shaded Air (but I have never yet made that Experiment when the Heat of the shaded Air was above 88): When we are therefore exposed in the Streets to the Sun in Summer, we inspire Air from 4 to 28 Degrees warmer than the Heat of the human Body.

The Thermometer, when buried in the Sands of the Streets, when the Heat of the shaded Air was 88, rose in 5 Minutes to 108, tho' there was at the same time a moderate Wind.

In *June* 1738, when the Heat of the shaded Air was 98, the Thermometer sunk one Degree in my Arm-pits; but continued at 98 in my Hand and Mouth: From which we see what little Concern the Air has in cooling the Blood in the Lungs. Two Men who were then in the Streets (when the

†

Heat

Heat was probably 124 or 126 Degrees, as the shaded Air's Heat was then 98) dropp'd suddenly dead; and several Slaves in the Country, at Work in the Rice-Fields, shared the same Fate. I saw one of the Men immediately after he died; his Face, Neck, Breast, and Hands, were livid.

From the barometrical Table it appears, that the Barometer's mean Altitude, taken from its greatest and least Height, is 30.09 Inches; and that its Range is only 1.22 Inch. Wherefore our Atmosphere varies only $\frac{1}{27}$ Part in its Weight. In the warm Months, the mean barometrical Station, taken from its greatest and least Altitudes in these Months, is 30.09 Inches; and I have never yet seen its Range in these Months exceed $\frac{1.8}{100}$ Parts of an Inch: Therefore the Changes of our Atmosphere's Weight, in the warm Months, will have but little Effect upon human Constitutions, as the Difference between its greatest and least Pressure is but $\frac{1}{2}$ Part of that in cold Climates, where the Range of the Barometer is three Inches. May not the great Height of the Barometer in the warm Months in this Climate, proceed from the vast Quantity of Water, which is at that time supported in our Atmosphere, as the Exhalation is then very great; or may it not proceed from the Rarefaction of the Mercury? For the Weight of the mercurial Column, at equal Altitudes, will be different under different Degrees of Heat; and the Mercury may therefore be supported at equal Heights by Columns of Air of unequal Weights.

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Heat

Heat was probably 124 or 126 Degrees, as the Shaded Air's Heat was then 98) dropp'd suddenly dead; and several Slaves in the Country, at Work in the Rice-Fields, shared the same Fate. I saw one of the Men immediately after he died; his Face, Neck, Breast, and Hands, were livid.

From the barometrical Table it appears, that the Barometer's mean Altitude, taken from its greatest and least Height, is 30.09 Inches; and that its Range is only 1.22 Inch. Wherefore our Atmosphere varies only $\frac{1}{27}$ Part in its Weight. In the warm Months, the mean barometrical Station, taken from its greatest and least Altitudes in these Months, is 30.09 Inches; and I have never yet seen its Range in these Months exceed $\frac{1.8}{100}$ Parts of an Inch: Therefore the Changes of our Atmosphere's Weight, in the warm Months, will have but little Effect upon human Constitutions, as the Difference between its greatest and least Pressure is but $\frac{1}{7}$ Part of that in cold Climates, where the Range of the Barometer is three Inches. May not the great Height of the Barometer in the warm Months in this Climate, proceed from the vast Quantity of Water, which is at that time supported in our Atmosphere, as the Exhalation is then very great; or may it not proceed from the Rarefaction of the Mercury? For the Weight of the mercurial Column, at equal Altitudes, will be different under different Degrees of Heat; and the Mercury may therefore be supported at equal Heights by Columns of Air of unequal Weights.

It appears, from the barometrical Table, that our Easterly or northerly Winds elevate the Mercury, and that our Southerly or Westerly Winds depress it; and I have as yet never observed the contrary. I beg you will be good enough to excuse this long Epistle, and believe that it comes from one who has a sincere Regard to the Improvement of Natural Knowledge, and one who is,

S I R,

Your most humble Servant,

John Lining.

A TABLE

A TABLE of the highest and lowest Stations of Fahrenheit's Mercurial Thermometer in the shaded Air, with the mean meridian and nocturnal Heat, taken after Dr. Jurin's Method.

	1738 Highest	1739 Lowest	1740 Highest	1742 Lowest	1738 Mean Altit. about 10 p. m. in the Heat of the Day	1739 Mean Altit. about 10 p. m. in the Heat of the Day
January	71.36	69.19	67.30	68.34	60.52	53.45
February	72.34	75.38	75.27	68.25	57.51	63.55
March	74.32	79.40	80.34	77.32	62.56	65.59
April	86.50	84.50	83.51	88.59	75.65	69.65
May	91.52	86.62	87.56	88.62	79.70	77.72
June	98.59	87.69	90.66	90.65	87.77	81.74
July	91.70	89.68	91.70	95.73	85.77	82.75
August	89.64	87.60	90.67	93.60	82.75	82.75
September	83.62	88.59	84.56	86.43	76.71	71.68
October	74.41	79.43	73.35	78.42	65.59	68.60
November	70.37	69.30	67.32	66.32	57.51	57.51
December	67.28	70.30	69.21	69.34	53.50	59.53
The Means					70.63	69.66

A TABLE of the highest and lowest Barometrical Stations; with the Directions which the Wind then had.

x A Northerly or Easterly Wind } preceded or succeeded.
 a A Southerly or Westerly Wind }

Jan.						30.42	N	29.82	SW	.60
Feb.						30.38	NE	29.68	S	.70
March						30.26	SE	29.58	S	.68
April	30.42	E	29.42	W	.94	30.38	Wx	29.78	WSW	.60
May	30.23	NE	29.85	S	.38	30.34	E	29.80	W	.55
June	30.20	NE	29.85	W	.35	30.30	E	29.98	SW	.32
July	30.13	SSWx	29.83	SW	.30	30.28	E	30.00	SW	.38
Aug.	30.18	E	29.81	SW	.30	30.30	NE	29.98	SW	.40
Sept.	30.33	NNE	29.85	SE	.48	30.38	E	29.88	NW	.50
Oct.	30.33	E	29.82	WNW	.50	30.45	E	29.68	W	.77
Nov.	30.58	N	29.72	S	.86	30.35	NE	29.58	W	.77
Dec.	30.60	N	29.92	W	.67	30.58	N	29.75	NNW	.83

Jan.	30.70	N	29.50	NW	.20	30.46	NNE	29.76	W	.70
Feb.	30.55	N	29.85	W	.70	30.54	NNE	29.72	WSW	.82
March	30.50	SE	29.65	W	.85	30.40	ENE	29.60	W	.80
April	30.32	E	29.75	N	.57	30.48	E	29.58	W	.90
May	30.28	E	29.85	S	.43	30.30	Sx	29.90	SW	.40
June	30.18	Sx	29.86	S	.32	30.28	ESE	29.90	NE	.38
July	30.08	SSEx	29.85	SSW	.23	30.22	W	29.98	SW	.24
Aug.	30.26	E	29.85	W	.41	30.25	NE	29.95	N	.30
Sept.	30.28	NE	29.85	NE	.43	30.36	NE	29.86	S	.50
Oct.	30.32	NNE	29.72	SW	.60	30.50	N	29.95	W	.55
Nov.	30.52	N	29.72	S	.79	30.55	NNW	29.73	SW	.82
Dec.	30.60	ENE	29.86	SW	.74	30.58	NNE	29.65	WNW	.93

*A TABLE of the Depth of Rain, in Inches and
milleſimal Parts, which fell in Charleſtown.*

	1738	1739	1740	1741	1742
January	1 097	2 310	4 873	4 492	2 189
February	4 416	2 875	3 084	4 615	1 650
March	4 532	5 609	1 141	5 713	5 203
April	1 082	0 195	1 092	1 308	0 918
May	3 127	5 120	5 612	4 841	5 898
June	1 567	15 839	4 648	5 538	3 250
July	10 660	5 452	3 013	3 399	1 252
Auguſt	4 104	12 211	7 301	7 144	7 647
September	10 792	4 834	3 200	6 734	2 895
October	1 358	6 593	1 258	3 399	0 759
November	2 656	1 235	1 848	2 964	3 388
December	3 877	3 689	2 736	1 919	0 957
Total Depth	149.268	155.962	139.806	152.066	136.006

	1743	1744	1745	The Means	1746
January	3 172	1 994	0 863	2 624	1 144
February	2 435	3 063	7 739	3 735	2 701
March	0 621	0 582	3 229	3 329	1 628
April	5 292	2 866	3 842	2 074	1 128
May	2 535	2 871	1 832	3 979	3 988
June	1 903	5 814	9 510	6 009	4 109
July	7 738	8 437	6 771	5 840	9 895
Auguſt	3 767	4 202	9 339	6 964	6 114
September	4 686	5 657	0 754	4 944	0 932
October	1 672	1 595	2 962	2 450	
November	3 220	1 562	0 682	2 194	
December	2 706	9 680	2 623	3 523	
Total Depth	39.747	48.323	50.146	47.666	

XIII. *An Abstract of the Bills of Mortality in Bridge-Town in Barbados for the Years 1737 ——— 1744. communicated by the Rev. Mr. John Clark.*

*Read May 26.
1748.*

Bridge-Town, Barbadoes.

<i>An. Dom.</i>	<i>Born</i>	<i>Males</i>	<i>Females</i>	<i>Baptiz'd</i>	<i>Buried</i>
1737	52	26	26	77	208
1738	81	41	40	106	250
1739	91	44	37	119	244
1740	91	49	42	123	242
1741	68	33	35	95	268
1742	87	42	45	130	296
1743	92	43	49	126	252
1744	89	46	43	120	166
	<hr/> 651	<hr/> 334	<hr/> 317	<hr/> 896	<hr/> 1919

XIV. *The Elements of a Short Hand, by Samuel Jeake Esq;.*

*Read May 26.
1748.*

A Succession of new Short-Hands published without the Reason of their Construction, having put me on forming a Method founded on Nature, the only Guide to Perfection, I settled an Alphabet in the following Manner.

Having





Having taken in a Book that lay by me a Paragraph as clear of the principal Idea of the Book as any I could find, consisting of near a thousand Letters, I enumerated the Repetitions of each of them, and wrote them down; and thereby made the following Table of the Number of Times each Letter was repeated in 1000. 'Tis true, it cannot be said the Repetitions will be exactly the same in every thousand Letters that may be taken either in the same Book or another; but whoever will enumerate them will not find Difference enough to be of Consequence.

The TABLE.

<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	<i>g</i>	<i>h</i>	<i>i</i>	<i>k</i>	<i>l</i>	<i>m</i>	<i>n</i>	<i>o</i>	<i>p</i>
81	20	23	45	99	18	18	54	78	3	36	15	66	83	12
<i>q</i>	<i>r</i>	<i>s</i>	<i>t</i>	<i>u</i>	<i>w</i>	<i>x</i>	<i>y</i>	<i>z</i>						
0	50	61	95	50	25	0	23	1						

After having made this Table, I consider'd with myself, that there were in Nature no more than eight simple Characters; four whereof are right, and the other four are crooked Lines.

The four right Lines are first the perpendicular Line |, and secondly the Line of Level —; which make the two Sides of a Square. Secondly the oblique Line / ascending from the left to right, and the oblique Line \ descending from left to right, making the two Sides of the Rhomb; which is the Figure of the Diamonds on the Cards.

The four crooked Lines are only the Semicircle when the Diameter is either above or below it, or on the right or left Hand of it as,    .

All

All Characters whatever must be made up of these, and from their Composition, which introduces Ambiguity of Signification, arises the Difficulty of reading a Short Hand, which uses the simple Characters for some Letters, and compound Characters for other Letters; or, which is as bad, for Words.

This Difficulty, being unavoidable in a Short Hand of more than eight Letters, making it appear that 8 was the Number of Letters a short Hand ought not to exceed, I considered it in the following Light.

1. If *a, e, i, o,* and the Aspirate *h,* be suppress'd, there will be 19 Letters only remaining to be represented by 8 Marks.
2. If *c, s, x, z,* which have a Sound much alike, be represented by one Character, there will remain 15 Letters to be represented by the other 7 Marks.
3. If *e, g, k, q,* which have a Sound not very different, be represented by one Character, there will remain 12 Letters to be represented by 6 Marks.
4. If *b, p, f* be represented by one Mark, there will remain 9 Letters to be represented by 5 Marks.
5. If *d, t* be represented by one Mark, only 7 Letters remain to be represented by 4 Marks.
6. If *l, r* be represented by one Mark, only 5 Letters remain to be represented by 3 Marks.
7. If *m, n,* are represented by one Mark, only 3 Letters remain to be represented by 2 Marks.
8. If *u, w,* are represented by one Mark, there will remain one Mark to represent *y* the only Letter hitherto unmentioned.

Writing

Writing with Suppression of the Vowels hath been always admitted into short Hands of all sorts, because the Consonants are look'd upon as radical Letters, which indeed they ought to be. I shall suppress *h*, as being not radical.

All short Hands are subject to Ambiguity; for there being but 8 Marks to represent 24 Letters; and those 8 being used for 8 of them in the Short Hand Alphabets, the other Letters must be described by Characters compounded of these 8.

The ranging of the Letters into Classes, as is done here, will hardly introduce a greater Ambiguity than all short Hands are subject to. So that this Method cannot be reckoned more puzzling to a Reader than any of the rest.

1. The Repetitions of *d* being 45, and of *t* 95, amount to 140, for the Repetition of this Class.

2. The Repetitions of *l*, being 36, and *r* 50, amount to 86, for the Repetition of this Class.

3. The Repetitions of *m*, being 15, and *n* 66, amount to 86, for the Repetition of the third Class.

4. The Repetitions of *u*, being 50, and of *w* 25, give 75, for the Repetition of the fourth Class.

5. The Repetitions of *e*, when of the Nature of *s*, being about half its Number in the Table, may be reckoned 10, those of *s* 61, those of *x* 0, and those of *z* 1, give 72, for the Repetitions of the fifth.

6. The Repetitions of *b*, being 20, of *f* 18, and of *p* 12, give 50, for the Repetitions of the sixth Class.

7. The Repetitions of *c* before *a*, *o*, *u*, being about 13, of *g* 18, of *k* 3, and of *q* 6, give 34, for the Repetition of the 7th Class.

8. The Repetition of *y*, being 23, gives 23 for the Repetition of the 8th Class.

By a little Reflection it will appear, that the Marks applicable to these Classes are in some measure determin'd. For a right Line taking up less Time than a crooked Line in its Description, it is plain the four first Classes must be referred to the four right Lines; and the four circular Parts to the remaining four last Classes.

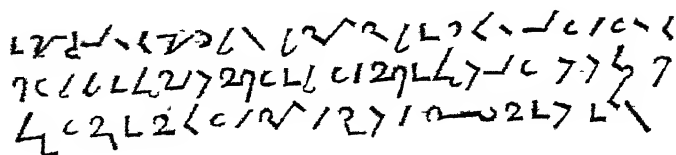
But the right Lines are indifferent to all the first four Classes, and the circular Parts to the four last Classes, for the Reason just mentioned. So that so much as relates to the fixing the particular right Line to represent the particular Class, is at the Liberty of the Inventor of a Short-Hand, to adjust agreeable to his own Fancy: And the same is true of the circular Parts. Thus any one may perceive how far the Fancy of a Short-Hand Maker is properly bounded or at Liberty.

I shall take notice of one shortening Rule; which is that of increasing the Dimensions of a Line, when the Letter must be repeated successively; as in *Man*, *rare*, and the like Cases. This is a good Rule of Mr. *Weston*.

An Alphabet according to the Classes.

<i>dt.</i>	<i>lr.</i>	<i>mn.</i>	<i>ww.</i>	<i>csxz.</i>	<i>bfp.</i>	<i>cgkq.</i>	<i>y</i>
/	—	\		C	3	6	0
				Z z			A

A Practice on the Lord's Prayer.



Which being expressed in Letters of the common Alphabet, will certainly convince the Reader how easily a Language may be read, tho' the Vowels are omitted. *e. g.*

ur fir we rt n un, llwd b ty nm, ty kndm cm, ty will b du n rt
s t e n un,
gv s ts dy ur dly brd, nd frgv s ur dts, s w frgv ur dtrs, nd ld s nt
nt tmptm, bt
dior s frm wh, fr tn s t kndm, t pwr, nd t glry, fr ur nd ur, mm.

As to the Introduction of Marks for representing Words in a Short-Hand, I shall not now say any thing more about it, than that all Short-Hands, this as well as any other, is equally fufceptible of them.

The Advantages of this Short-Hand in the State exhibited, when perfectly learned, so as to be wrote readily, will appear to be,

1. That, by Suppression of *aeioh*, or $\frac{400}{1000}$, only $\frac{6}{10}$ of the Time of writing ordinary Long-Hand is necessary to write this.
2. That the simple Strokes representing the Consonants, not taking up above half the Time of writing the Consonants, only half of $\frac{6}{10}$, or $\frac{3}{10}$ of the Time of any thing wrote in Long-Hand is necessary for writing this.

2. That the simple Strokes representing the Consonants, not taking up above half the Time of writing the Consonants, only half of $\frac{6}{10}$, or $\frac{3}{5}$ of the Time of any thing wrote in Long-Hand is necessary for writing this.

3. Right Lines not taking up more than $\frac{2}{3}$ of the Time of Description of crooked Lines, as the Diameter is $\frac{2}{3}$ of the Semiperiphery, it appears, if only right Lines were used, these $\frac{1}{10}$ would be reduced to $\frac{2}{10}$, by the Subtraction of $\frac{1}{3}$ of $\frac{3}{10}$. But, because the Number of right Lines, all things consider'd, should not be reckon'd but about double the Number of crooked ones, only $\frac{2}{3}$ of $\frac{2}{10}$ can be taken from the $\frac{1}{10}$; that is to say, the Time taken up in writing this Hand will be $\frac{9}{30} - \frac{2}{30} = \frac{7}{30}$ of the Time taken up in writing of the common Long-Hand, or or less than the $\frac{1}{4}$ of the Time.

As I have shewn all the Principles on which Short-Hands can be constructed to Advantage, I have no need to compare this with any other; because I have enabled every Reader to judge of them, by shewing within what Limits all Improvements are bounded.

XV. *An Account of a Treatise by Wm. Brownrigg M.D. F.R.S. intituled, "The Art of making common Salt, as now practised in most Parts of the World; with several Improvements proposed in that Art, for the Use of the British Dominions;" abstracted by W. Watfon F. R. S.*

Gentlemen,

Read June 15. 1748. **I** RECEIVED your Commands to lay before you an Extract of our worthy Brother Dr. *Brownrigg's* Book; which, though at

Z z 2

all

all times ready to execute whatever you think proper to charge me with to the utmost of my Abilities, I engaged in the more readily, from the Pleasure and Instruction I had already received from the Perusal of that excellent Work, in which its Author has eminently distinguished himself both as a Chemist, and as a Philosopher.

This Work consists of 295 Pages in 8^m, exclusive of the Preface, and of 6 Copper-Plates, exhibiting different Views of Salt-Houses, Instruments, &c. necessary to the Preparation of Salt. It is enriched likewise with Notes of great Importance to the Work, not only of the Author, but also from the *Philosophical Transactions*, *Medical Essays*, *Memoirs of the Royal Academy of Sciences at Paris*, *Pliny*, *Agricola*, *Alonso Barba*, *Ramusio*, *Boyle*, *Hoffman*, *Lister*, *Herrera*, *Dampier*, *Baccius*, *Pomet*, *Marsilli*, *Plot*, *Scheuchzer*, *Hales*, *Rastel*, *Leigh*, *Boerhaave*, *Shaw*, and others.

Amongst the vulgar Arts, that of preparing Sea-Salt for the Uses of Mankind hath been thought worthy the notice of many great and learned Men, as well antient as modern. Thus many things relating to this Art are recorded by *Cato* and *Pliny*, *Agricola* and *Hoffman*, to whom our Author is much indebted for those Memoirs that have been transmitted to us, relating to its History. Had those great Men been as diligent in improving this Art, as they were in recording the Improvements made therein by others, there would not now have been Occasion to remark, that, after the Practice of so many Ages, an Art so simple, and withal so necessary, hath not yet been brought to any Degree of Perfection.

That

That this Art was capable of great Improvements, especially as practised in *Great Britain*, was the Sentiment of this *Society* soon after its Institution; at which time the Members thereof were very intent upon bringing it to a greater Perfection; as may be gather'd from the Inquiries and Suggestions of Dr. *Beal*, and the Histories of several Methods of making Salt, which then were published by the *Society*. And although the *English* have, since that time, considerably improved their Method of boiling Salt; yet this Art is still practised with greater Skill and Success by the *Dutch*, as the superior Goodness of the Fish, cured with their Salt, doth sufficiently prove.

The Commons of *Great Britain*, having taken into Consideration the great Importance of this Art, judg'd some Improvements propos'd therein worthy their Regard and Encouragement; well knowing, that, could this be brought to the same Perfection in *Britain* as in some neighbouring Countries, large Sums of Money might be saved in the Nation, which are now paid to the *French* and others; its Fisheries improved, and its Navies and Commerce, and many of its richest Colonies, would no longer depend upon its Enemies for one of those Necessaries, without which they cannot be supported.

These Considerations have induc'd our Author to give a brief Account of the various Methods of making Salt, which are now used in *Great Britain*, and in other Countries, where this Art is practised with more Success; and also to attempt several further Improvements for the Use of the *British* Dominions. How far he has succeeded in these Attempts,

tempts, will best appear, if the Public shall think the following Proposals so far worthy their Attention, as to merit a fair and impartial Trial. The principal Conclusions, deduced from a Variety of Observations and Experiments, are as follows: 1. That, by the Methods here proposed, an excellent Bay-Salt may be made in *Britain* in very large Quantities, so as to be afforded cheaper than at the Prices paid for foreign Salt; and that the *British* Colonies in *America* may very commodiously be supplied with Bay-Salt of their own Manufacture, without having recourse for it to the *French*, *Spaniards*, and *Portuguese*. 2. That, by the Methods here proposed, an excellent kind of refined white Salt may be made in *Britain*, as well from Sea-Water and Rock-Salt, as from natural Brine, in any Quantity wanted, so as to be afforded cheaper than foreign Bay-Salt; and which will also be better for curing Fish, Flesh, and other Provisions.

In forming these Conclusions, an impartial Regard has been had to Truth, without attending to the private Advantage of any particular Set of Men. The Sense of this, together with a Desire of promoting the public Advantage, has induced our Author to communicate the following Sheets at this time, although by deferring the Publication some time longer he might have made them possibly more accurate; because, besides other Considerations of no small Import, an Opinion has prevailed, that the establishing of Fisheries in the North of *Scotland* would be the best Means of affording an useful Employment to more unciviliz'd Inhabitants of that Part of the Kingdom, for carrying on of which they are most commodiously situated.

What

What Mr. *Lowndes* * hath lately done towards the Improvement of Brine-Salt, may, perhaps by some, be thought to supersede the Necessity of further Attempts for improving and extending our Salt Manufacture. Dr. *Brownrigg* is very far from depreciating the Endeavours of that Gentleman, which have met with Parliamentary Encouragement; and had his Discovery appeared to the Doctor sufficiently complete and extensive, he would not have given the Public and himself this Trouble. He makes no Doubt but that the Specimen of Salt, which Mr. *Lowndes* exhibited before the College of Physicians, was a strong and pure Salt, since such it appeared to that most learned Body. Whether the Alum mixed with it (agreeable to the antient Practice of the *Cheshire* Salt boilers) contributed any thing to its Goodness, is more properly consider'd hereafter. It is only necessary here to observe, in Justification of the present Undertaking, that Mr. *Lowndes's* Method of making Salt for curing Provisions, doth not appear to be the best that may be put in Practice; since our Author hopes to shew, that, by other Methods, a purer and a stronger Salt may be made, and at a less Expence. Neither is his Method so general and extensive as seems to be required for the public Good; since Mr. *Lowndes* confines it almost intirely to boiled Brine-Salt; and hath given no Directions concerning the Preparation of Bay-Salt. He indeed proposes to meliorate the *British* Sea-Salt, but seems to despair of preparing a Salt either from Sea-Water, or *English* Rock-Salt,

fit

* Mr. *Lowndes's* Process is inserted in this Work. See p. 104 et seq.

fit for the Uses of the Navy or Fisheries; altho' the *Dutch* Salt, which is the strongest and purest boiled Salt now made, is entirely a marine Salt, and even the Brine, of which Mr. *Lowndes* makes his Salt, is only a Solution of the *English* Rock-Salt, often in very impure Water, as is well known to the Naturalists.

Our Author, treating of Salt in general, takes notice of the Excellence and Usefulness thereof; and that it hath pleased the *Author* of Nature to provide Mankind therewith in such Abundance, that there are few Countries which do not afford vast Quantities of Rock or fossil Salt. Mines of it have been long discovered and wrought in *England, Spain, Italy, Germany, Hungary, Poland*, and other Countries in *Europe*. Moreover the Sea affords such vast Plenty thereof, that all Mankind might thence be supplied with Quantities sufficient for their Occasions. There are also innumerable Springs, Ponds, Lakes, and Rivers impregnated with common Salt, from which the Inhabitants of many Countries are plentifully supplied herewith.

In some Countries, which are remote from the Sea, and have little Commerce, and which are not blessed with Mines of Salt, or salt Waters, the Necessities of the Inhabitants have forced them to invent a Method of extracting their common Salt from the Ashes of Vegetables.

In short, this Salt is dispersed all over Nature; it is treasured up in the Bowels of the Earth; it impregnates the Ocean; it descends in *Rains; it fertilizes

* See *Boyle* on the Saltiness of the Sea.

tilizes the Soil ; it arises in Vegetables ; and from them is convey'd into Animals ; so that it may well be esteemed the universal Condiment of Nature.

Naturalists, observing the great Variety of Forms under which this Salt appears, have thought fit to rank the several Kinds of it under certain general Classes, distinguishing it most usually into Rock or fossil Salt, Sea-Salt, and Brine or Fountain-Salt : To which may be added others of those muriatic Salts, which are found in vegetable or animal Substances. These several Kinds of common Salt often differ from each other in their outward Form and Appearance, or in such accidental Properties as they derive from the heterogeneous Substances with which they are mixed ; but, when perfectly pure, they have all the same Qualities ; so that Chemists, by the exactest Inquiries, have not been able to discover any essential Difference between them. In this our Author agrees with the celebrated † *Hoffman*. Leaving therefore these Divisions to those whom they may concern, it may for the present Purpose be more proper to distinguish common Salt after a different Manner into the three following Kinds ; *viz.* into Rock or native Salt, Bay-Salt, and white Salt.

By

* *Hoffman* de salinibus *Hallens.* cap. viii.

Ut igitur nostra hac de re innotescat sententia, hanc interponimus ; sicuti in tota universi hujus orbis compage, una tantum est aqua, unus per fermentationem paratus spiritus ardens, unus Mercurius, unum volatile sal, unum acidum nitrosum ac vitriolicum sal ; ita, pari ratione unum idemque sal commune est. Sed quum plures alienæ, terreæ, lapidosæ, sulphureæ, calcaris minérales ac pingues particule cum hisce corporibus connubium ineant, diversa exinde emergit eorum indoles ; et sal commune idem semper obtineret ingenium, siquis pingues terreas, calcareasque partes ab illo artificiose segregaret.

By Rock-Salt*, or native Salt, is understood all Salt dug out of the Earth, which hath not undergone any artificial Preparation.

Under the Title of Bay-Salt may be ranked all Kinds of common Salt extracted from the Water, wherein it is dissolved by means of the Sun's Heat, and the Operation of the Air; whether the Water, from which it is extracted, be Sea-Water, or natural Brine drawn from Wells and Springs, or Salt Water stagnating in Ponds and Lakes.

Under the Title of white Salt, or boiled Salt, may be included all Kinds of common Salt extracted by Coction from the Water wherein it was dissolved; whether this Water be Sea-Water, or the salt Water of Wells, Fountains, Lakes, or Rivers; or Water of any sort impregnated with Rock-Salt, or other Kinds of common Salt.

The first of these Kinds of Salt is in several Countries found so pure, that it serves for most domestic Uses, without any previous Preparation, Triture excepted. But the *English* fossil Salt is unfit for the Uses of the Kitchen, until by Solution and Coction it is freed from several Impurities, and reduced to white Salt. The *British* white Salt also is not so proper as several Kinds of Bay-Salt for curing Fish, and such Flesh-Meats as are intended for Sea Provisions, or for Exportation into hot Countries. So that, for these Purposes, we are obliged, either wholly

* By *Rock-Salt*, or *Sal Rupium*, the antient *Chemists* mean Salt adhering to the Rocks above the high Water Mark, being there lodged by the Spray of the Sea, evaporated by the Heat of the Sun; which is the purest Salt of all for chemical Uses, and is to be had off the Rocks of *Sicily*, and several Islands in the *West Indies*.

wholly or in part, to use Bay Salt, which we purchase in *France, Spain*, and other foreign Countries. To remedy these Inconveniencies this Treatise was wrote, in order to shew how the Subjects of *Great Britain* may be supplied with Salt of their own Manufacture, fit and sufficient for all their Occasions.

In order that the Methods here proposed might be better understood, and that the Reasonableness of them might more fully appear, the Author thought it necessary to premise a brief Account of the several Ways of preparing Bay-Salt, as well as white Salt, as far as they came to his Knowledge. From this History may be formed a Judgment, how far the Methods now in Use are proper, in what deficient, where erroneous, and how they may be improved.

Bay-Salt in general may be divided into two Kinds. First, Bay-Salt, drawn from Sea-Water, as is practised in *France, Spain, Portugal*, and many other Countries. Secondly, Bay-Salt extracted from salt Springs, Ponds, and Lakes; as at *Cape de Verd* Islands, *Tortuga*, and other Places. Of these the first is imported in large Quantities into *Great Britain* and *Ireland*: Our *American* Colonies, in Times of Peace, are chiefly supplied with the latter; but in Time of War they have large Quantities of Bay-Salt from *Lisbon*, and other Parts of *Portugal*.

Bay-Salt is prepared in a Manner the most simple and easy, when the Water of Ponds and Lakes impregnated with Salt is totally exhaled by the Force of the Sun and Air, and the Salt is left concreted into a hard Crust at the Bottom of the Lake or Pond. Of Salt thus prepar'd we have Instances

in many Parts of the World, as in the *Podolian* Desert near the River *Borysthenes* on the *Russian* Frontiers towards *Crim Tartary*, in the Kingdom of *Algiers*, and in other Parts of the World.

Bay-Salt is also drawn from the Brine of Ponds and Lakes, and our Author gives us an Account of the preparing it in this manner in the *Cape de Verd* Islands. This Account was collected chiefly from the Relations of several Persons of Credit, who themselves assisted in making Salt in these Islands. He also takes notice of the Bay-Salt made at *Tortugas*, and other Places in *America*. He describes likewise the Manner of making marine Bay-Salt in *France*, and other Parts of *Europe*. For the Particulars of these Operations I must refer you to the Work itself; and only take notice, that every kind of Bay-Salt is prepared without artificial Heat, and by only exposing the Brine under a large Surface to the Action of the Sun and Air, by which, in proportion to the Strength of the Brine, and to the different Temperature of Climate and Season, the Salt chrySTALLIZES into what we call Bay-Salt, and comes under different Appearances to us from different Places, which arise principally from the Cleanliness and Care of the Artist.

Our Author, when treating of white Salt in general, acquaints us, that although Salt is made, in warm Climates, with the greatest Ease, and at the least Expence, by the Heat of the Sun, after the Methods already described; yet, in several Countries, where Bay-Salt might be conveniently made, they prepare all their Salt by culinary Fires. Thus in *Austria*, *Bavaria*, and many other Parts of *Germany*, and also in *Hungary*, and even in some Parts of *Italy*, they constantly boil the Water of their

their salt Springs into white Salt. But in other Parts of *Europe*, as in *Britain*, and in the Northern Parts of *France* and *Germany*, an erroneous Opinion long prevailed, that the Heat of the Sun was not there sufficiently intense, even in the Summer Season, to reduce Sea-Water, or Brine, into Bay-Salt. And all Arguments would probably have been insufficient to remove this Prejudice for the *English*, had not the contrary been fully proved by Experiments, which were first accidentally made in *Hampshire*. However, the Method of making Salt by Coction will probably still continue to be practised in *Britain*; as the Salt so prepared is for several Uses preferable to Bay-Salt; and when prepared after a particular Manner, is preferable to common Bay-Salt, even for curing Provisions, as the Practice of the *Hollanders* sufficiently testifies: So that the due and right Preparation of white Salt seems very deserving of the Notice and Regard of the Public.

White Salt, as it is prepared from various saline Liquors, may therefore be distinguished into the following Kinds:

1. Marine boiled Salt, which is extracted from Sea-Water by Coction.
2. Brine or Fountain-Salt, prepared by Coction from natural Brine, whether of Ponds or Fountains.
3. That prepared from Sea-Water, or any other kind of Salt-Water, first heightened into a strong Brine by the Heat of the Sun, and the Operation of the Air.
4. That prepared from a strong Brine or *Lixivium* drawn from Earths, Sands, or Stones impregnated with common Salt.
5. Refined Rock-Salt, which is boiled from a Solution of fossil Salt in Sea-Water, or any other kind of salt Water, or pure Water.
6. Lastly, Salt upon Salt, which is Bay-Salt dissolved in Sea-Water,

or

or any other salt Water, and with it boiled into white Salt; and under these Heads may be ranked the several kinds of boiled Salt now in Use. Our Author has given us an exact History of the Manner of preparing these different kinds of Salt, as practised in different Places, with miscellaneous Observations and Cautions relating to their respective Processes, for which in the general I must refer you to the Work itself: But the making Salt upon Salt deserves more particular Attention; as the Author, being under no Tie of Secrecy, has revealed to us the Method of making in *Holland* and *Zealand* that strong and pure kind of Salt, with which they cure Herrings, and all other Provisions for long keeping; which gives the *Dutch* a great Advantage over all other Nations in the Herring-Fishery; since Fish preserved with this Salt look much cleaner and fairer than those that are cured with Bay-Salt, and keep much better than those preserved with any other kind of white Salt.

From the Process whereby white Salt is made from Sea-Water by Coction, it appears, that Sea-Water, besides common Salt, contains several other Ingredients; some of which are separated before the common Salt falls, and others remain in the Bittern, after all the Salt is extracted. Our Author has given a full and circumstantial Account of these in an express Chapter, under the Appellation of Memoirs for an Analysis of Sea-Water.

The Salt-Boilers, and particularly those who prepare Brine-Salt, have long been accustomed to make use of various Substances, which they call Additions or Seasonings, and mix them with the Brine while it
is

is boiling, either when they first observe the Salt begin to form, or else afterwards during the Time of Granulation. These Additions they use for various Purposes. First, to make the Salt grain better, or more quickly form into Chrystals. Secondly, to make it of a small fine Grain. Thirdly, to make it of a large firm and hard Grain, and less apt to imbibe the Moisture of the Air. Fourthly, to render it more pure. And lastly, to make it stronger, and fitter for preserving Provisions.

These Additions, most commonly used to answer the above-mention'd Purposes, are Wheat-Flour, Resin, Butter, Tallow, new Ale, stale Beer, Bottoms or Lees of Ale and Beer, Wine-Lees and Alum. Wheat-Flower and Resin are used for the Property they possess of making the Salt a small Grain. Butter, Tallow, and other unctuous Bodies are commonly applied, as they are said to make the Brine chrySTALLize more readily; for which End some Salt-Boilers more particularly prefer the Fat of Dogs: But others have little to plead for their using these Substances, but immemorial Custom: How far they have the Effects ascribed to them can only be determined by Experiments, as several Boilers, who formerly used them, now find they can make as good Salt without them. Wine-Lees, new Ale, stale Ale, the Lees of Ale and Beer are now generally rejected by the marine Salt-Boilers; except in the West of *England*, where the Briners, who use them, affirm that they raise a large Grain, and make their Salt more hard and firm, and some say that they make it chrySTALLize more readily. *Hoffman* prefers the strongest Ale; and *Plot* assures us, that
it

it makes the Salt of a larger or smaller Grain, according to the Degree of its Staleness. The only good Effects that fermented Liquors can have as an Addition, are probably owing to their acid Spirit, which may correct the alkaline Salts of the Brine, and so render the common Salt more dry and hard, and less apt to dissolve in moist Air. If therefore it should be thought necessary to use any of these Additions, in order to correct the alkaline Quality of the Brine, stale Ale, or *Rhenish* Wine*, ought to be chosen, as new Ale contains but little Acid.

Alum is an Addition long known and used in *Cheshire*, together with Butter, to make the Salt precipitate from some Sorts of Brine, as we are assured by Dr. *Leigh* in his Natural History of *Lancashire*, *Cheshire*, &c. who first taught the *Cheshire* Salt-Boilers the Art of refining Rock-Salt. As the bad Properties of their Salt proceeded from hard boiling, they found every Method ineffectual, until they had recourse to a more mild and gentle Heat. And as Alum hath been long disused amongst them, it is not likely, that they found any extraordinary Benefit from it; otherwise they would scarce have neglected it, and continued the Use of Butter. However Mr. *Lowndes* hath lately endeavour'd to revive its Use; asserting, that Brine-Salt hath evermore two main Defects, Flakyness and Softness; and to remedy these Imperfections, he tried Alum, which fully answered every thing he propos'd; for it restored the Salt to its natural cubical Shoot, and gave it a proper Hardness; nor had it any bad Effect whatever. But our Author is of Opinion, that whoever considers the Nature of Alum, will scarce expect

* Why not Malt-Vinegar?

peet such extraordinary Effects from it. Neither does it here seem wanted; for the Grains of common Salt will always be sufficiently hard, and of their natural Figure, large Size, and no-ways disposed to run by the Moisture of the Air, if formed by a gentle Heat, and perfectly free from heterogeneous Mixtures: So that the Goodness of Mr. *Lowndes's* Salt does not seem owing to the Alum, with which it is mixed, but chiefly to the gentle Heat used in its Preparation.

The *Dutch*, who have long shewn the greatest Skill and Dexterity in the Art of boiling Salt, make use of another Addition, which they esteem the greatest Secret of their Art. This is Whey, kept several Years till it is extremely acid; now first revealed by our Author to the *British* Salt-Boilers, but long held in great Esteem by the *Dutch*, for the good Effects it hath upon their Salt; which it renders stronger, more durable, and fitter to preserve Herrings, and other Provisions.

Bay-Salt, as well as white Salt, is of different Kinds, and possessed of different Qualities: With the different Kinds of these Provisions must be cured, according to the Uses for which they are designed. The *Dutch* indeed use no Salt for curing Provisions, besides their own refined Salt. With it they can preserve Flesh and Fish of all Kinds as well as with the strongest Bay-Salt; and chuse to be at the Expence of refining Bay Salt, rather than to defile their Provisions with the Dirt and other Impurities, with which it commonly abounds.

Salt, esteemed the best for curing Provisions, and for preserving them the longest time, is that which is the strongest and the purest. This may be known by

the following Characteristicks; *viz.* it is usually concreted into large Grains or Chrystals, which are firm and hard, and in respect to those of other Kinds of common Salt, the most solid and ponderous; it is not disposed to grow moist in a moderately dry Air, to which it has been exposed a considerable time; its Colour is white, and somewhat diaphanous; it hath no Smell; its Taste is truly muriatic, and more sharp and pungent than that of other Kinds of common Salt. It has, besides these, several other distinguishing Properties mentioned by our Author. The Salts, which approach nearest to this Degree of Perfection, are the best Kinds of Bay-Salt, and the strong *Dutch* refined Salt; but most of the Salt now made for Sale is very far from answering to these Characteristics.

Having related the various Methods of preparing Salt that now are in Use, as far as they are come to our Author's Knowledge, it appears, that this Art is not brought to such Perfection in the *British* Dominions as in several other Countries, the Salt there prepared being unfit for preserving many Kinds of Provisions. It remains now to shew, that this Want of a strong Salt of *British* Manufacture proceeds not from any Defect in Nature, but of Art; and that, if proper Skill and Industry be used in the *British* Dominions, and due Encouragement there given by the Legislature, such Improvements may be made in this Art, that not only *Great Britain*, but *Ireland* also, and the *British* Colonies in *America*, may be supplied with Salt of their own Manufacture, proper for curing all Kinds of Provisions, in Quantity sufficient for all their Occasions, in Quality equal, if not superior, to any foreign Salt

Salt now made, and at a moderate Price. These are Truths, which the Author hopes will appear evident from the Facts and Reasonings contained under the following Positions:

Lemma I. The Quantity of Water which annually falls in Rain, Snow, and Hail, is very different in different Parts of *Great Britain*; there commonly falling almost double the Quantity on the Western Coasts, that falls on the Eastern Coasts of that Island.

Lemma II. The Quantity of Rain which falls in *Lancashire*, during the four hottest Months of the Year, viz. *May, June, July, and August*, doth not at a Medium amount to more than a third Part of the Quantity of Water, which falls in Rain, Snows, and Hail, during the whole Year.

Lemma III. The Water which ascends in Vapours from the Sea very greatly exceeds that which descends thereon in Rain and other aqueous Meteors: But the Quantity of Water, which usually exhales from a given Part of the Ocean in a given Time, cannot with any Exactness be determined.

Lemma IV. The Quantity of Water which commonly exhales in *Great Britain* from shallow Ponds during the four hottest Months of the Year, greatly exceeds the Quantity of Rain which commonly falls on the Surface of those Ponds during the said Months.

From these *Lemmata*, which the Author has supported by the Observations, not only of himself, but of other learned Men, are deduced the following Propositions:

Proposition I. In several Parts of *England* large Quantities of Bay-Salt may be extracted from Sea-Water during the hottest Months of the Year, by receiving the Salt-Water into Ponds, and suffering its aqueous Parts thence to exhale by the Heat of the Sun, and the Operation of the Air and Winds.

Prop. II. In several Parts of *England* large Quantities of Bay-Salt may very commodiously be extracted from Sea-Water, after the same manner that is practised in *France*, and in other Parts of *Europe*.

Prop. III. Bay-Salt may be extracted in *England* from Sea-Water in larger Quantities, and with more Certainty, than by the foregoing Method, if Care be taken to preserve the Brine contained in the Salt-Pits from being diluted with Rains, and to promote the Evaporation of the Water by several artificial means, which may easily be put in Practice.

Prop. IV. In several Parts of *England* large Quantities of excellent Bay-Salt may with great Ease be made from the natural Brine of salt Springs, and also from Rock-Salt dissolved in weak Brine or Sea-Water.

Prop. V. Bay-Salt may be prepared in *England* by the foregoing Methods at a very moderate Expence, equal in Goodness to the best foreign Bay-Salt, and in Quantity sufficient for the Consumption of all the *British* Dominions.

Prop. VI. In several of the *British* Colonies in *America*, Bay Salt might, with little Expence and Trouble, be prepared from Sea-Water, in Quantities sufficient to supply the *American* Fisheries, and

and all other Occasions of those Colonies, so as to become a considerable Branch of their Trade.

The Author has supported all these Propositions with great Ingenuity ; but I cannot pass over in Silence the artificial means to promote the Evaporation of Sea-Water, mention'd in *Prop.* III. as well as to preserve the Brine contained in the Salt-Pits from being diluted with Rains. I therefore shall lay before you a short Account of these.

It will be proper, says our Author, to make all the Salt-Pits of the Marsh in one long Row extended from East to West, and for each Pit to make Covers of thin Boards, or rather of coarse Canvas, or Sail-Cloth, stretched on Frames of Wood and painted white. These Covers must all be fixed with Hinges to strong Posts and Beams on the North Side of the Pits ; so that they may be let down and drawn up with Cords and Pulleys, or by some other Contrivance, somewhat like Drawbridges. These Covers thus fixed may be let down over the Pits like a Shed or Penthouse in rainy Weather ; and in dry Weather may be erected almost to a Perpendicular, but inclining a little towards the South ; so as to form a Wall with a South Aspect. Thus these may serve a double Purpose, as Coverings for the Pits in wet Weather, and as Reflectors of the Sun's Heat upon them in dry Weather, and thus greatly promote the Evaporation of the aqueous Parts of the Brine. The Hinges on which the Reflectors turn may be fixed about eight or ten Inches from the Ground ; by which means, when the Reflectors stand upright, there will be an Opening left beneath them, through which the Air will continually

ally flow in a brisk Current, and greatly increase the Evaporation of the Water.

After having gone through that Part of Dr. *Brownrigg's* Work, which relates to Bay-Salt, we proceed to the Methods that Gentleman proposes for preparing and improving white Salt, which, if brought into Use, may probably be of Advantage not only to private Undertakers, but also to the Public. For it appears, that two very different Kinds of white Salt are required; the one for the Use of the Table, and the other as a Condiment for Provisions. Its Whiteness, Dryness, and the Smallness of its Grain, are the Properties which chiefly recommend the first Kind; and its great Strength and Purity the latter. It is this strong and pure Kind of white Salt, which is wanted in the *British* Dominions; and it is therefore our Author's principal Design here to consider how this Defect may be supplied; although at the same time Instructions are given how to prepare Table Salt, not only better in Quality, but also at a less Expence than it is now prepared by the common Methods. |

Lemma I. In the common Processes for making white Salt, the Salt is deprived of a considerable Part of its acid Spirit, by the violent Boiling used in its Preparation.

Lemma II. Most Kinds of white Salt are render'd impure by the Mixture of various heterogeneous Substances.

Lemma III. White Salt, by the violent Cotion commonly used in its Preparation, is render'd less fit for preserving Fish, Flesh, and other Provisions, than it would be if prepared with a more gentle Heat.

Lemma

Lemma IV. The heterogeneous Substances which are commonly mixed with white Salt, render it less proper for preserving Provisions, than it would be if separated from them.

After having fully consider'd the foregoing, our Author gives a Method of preparing a Kind of white Salt proper for curing Fish, Flesh, and other Provisions; likewise a Method of refining Salt; but for these I must refer you to the Work itself, as well as for the Tables, wherein the several Expences attending these Operations are minutely consider'd.

Most of the Facts referred to in these Disquisitions are such, as the constant Practice of those who make Salt sufficiently warrants us to rely upon for true and certain; or else, they are the Observations of judicious Salt-Officers, daily conversant in these Matters, or of curious and inquisitive Navigators, Merchants, Travellers, and Naturalists; or, lastly, the Experiments of many learned Physicians, Chemists, and Philosophers: The Truth of which several Facts, though many of them have long been published, hath never been called in Question. So that these Observations and Experiments may probably be more relied on by the Public, than if they had only been made by our Author; since they have the Testimony of many skilful and unprejudiced Persons, who could have no Notion of the Uses to which they have been here applied. If therefore the Arguments founded upon those Facts should be esteemed any-ways reasonable and satisfactory, the Author presumes to remark, that it might not be unworthy the Wisdom of the *British* Legislature to direct

direct a more full Inquiry to be made into a Matter of this Importance, and to order proper Works to be erected for making Bay-Salt, and for making and refining white Salt, and to put those Works under the Management of able and judicious Persons, to make exact and accurate Trials, in order to discover the best and cheapest Methods of doing them. And the Methods, which should be most approved of, might for the general Good be made public, and established by Law as a common Standard, to which all those who make Salt in the *British* Dominions should be obliged to conform.

However imperfect this Extract may appear, I must now beg your Indulgence for having taken up more of your Time than is usually allow'd to Works of this kind. I must plead in my Excuse the great, the National Importance of the Work itself, the masterly Manner with which the Subject-Matter is treated, as well as its falling in so exactly with that Institution, in which we are so desirous of distinguishing ourselves. The making and refining Salt must certainly be considered as one of those mechanic Arts, the History of which, as we are taught by the noble * *Verulam*, is a necessary Part of that Knowledge, that true Science of Nature, which is not taken up in vain and fruitless Speculations, but effectually labours to relieve the Necessities of human Life.

XVI.

* *Verulam de Aug. Scient.* lib. II. cap. 2.

XVI. *A Catalogue of the Immerfions and Emerfions of the Satellites of Jupiter, that will happen in the Year 1750, of which there are 173 of the Firft, 85 of the Second, 94 of the Third, and none of the Fourth, by reason of its great Latitude; in all 322. Computed to the Meridian of London from the Flamfteedian Tables: Corrected by James Hodgfon F. R. S. Mafter of the Royal Mathematical School in Chrift's-Hofpital.*

Note, Thofe that are marked with an Afterisk are vifible at London.

THE great Improvements made in the ufeful Sciences of Geography and Hydrography, by the Observations of the Eclipses of *Jupiter's* Satellites, are too well known to need any Account of them, or Encomium upon them. They were judged very proper for this Purpose by *Galileus* himfelf, who firft difcovered them, and all the Astronomers at that time, as being the moft certain, fure, and eafieft Method then known, and I may venture to fay even to this Time, for afcertaining the Difference of Longitudes between Places, how remote foever: And it is this that has encouraged Perfons to make conftant Observations of them: And that they may not neglect the frequent

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quent Opportunities that offer themselves for want of timely Notice, I formerly published in the *Philosophical Transactions** an annual Catalogue of all the Eclipses that would happen that Year, as I do now for the succeeding Year, and shall continue to do for the Time to come, if my Health will permit me: And if those Persons who shall make any Observations, will be so good as to communicate them to me, they will be gratefully received, as they will tend to discover the Errors of the Tables, which shall be my constant Endeavour to find out, so long as it shall please the divine Providence to enable me to do it.

ECLIPSES of the first Satellite of JUPITER.

Emerfions.				Emerfions.				Emerfions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
JANUARY.				21	8	42	16*	8	1	29	26
				23	3	10	58	9	19	58	13
1	21	30	11	24	21	19	49	11	14	27	26
3	15	58	11	26	6	8	20*	13	8	56	45
5	10	26	13	28	10	36	57	15	3	26	1
7	4	54	32*	30	5	5	34*	16	21	55	13
8	23	23	7	31	23	34	19	18	16	24	25
10	17	51	43					20	10	53	39
12	12	20	21	FEBRUARY.				22	5	22	17*
14	6	48	38*					23	23	52	17
16	1	17	5	2	18	2	59				
17	19	45	28	4	12	31	50				
19	14	13	50	6	7	0	39*				
								APRIL.			

* See N^o. 449, in the Year 1738. and others in the preceding Years.

ECLIPSES of the first Satellite of JUPITER.

Immersions.				Immersions.				Immersions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
APRIL.				JUNE.				8	10	2	43
25	2	13	23	1	6	10	22	10	4	31	8
26	20	42	15	3	0	38	33	11	22	59	40
28	15	10	54	4	19	6	44	13	17	28	12
30	9	39	33	6	13	34	56*	15	11	56	44*
				8	8	3	6	17	6	25	18
MAY.				10	2	31	17	19	0	53	53
2	4	8	17	11	20	59	25	20	19	22	33
3	22	36	52	13	15	27	38*	22	13	51	19
5	17	5	26	15	9	55	57*	24	8	20	5
7	11	33	57*	17	4	24	13	26	2	48	51
9	6	2	27	18	22	52	18	27	21	16	39
11	0	30	59	20	17	20	31	29	5	46	26
12	18	59	24	22	11	48	42*	31	10	15	17*
14	13	27	49	24	6	16	54	AUGUST.			
16	7	56	14	26	0	44	7	2	4	44	8
18	2	24	53	27	19	13	25	3	23	11	38
19	20	52	50	29	13	41	39*	5	17	40	38
21	15	21	4*	JULY.				7	12	9	38*
23	9	39	27	1	8	9	56	9	6	38	39
25	4	17	40	3	2	37	36	11	1	7	41
26	22	55	53	4	21	6	56	12	19	36	44
28	17	14	7	6	15	33	20*	14	14	5	53*
30	11	42	21					16	8	35	2

ECLIPSES of the first Satellite of JUPITER.

Emerfions.				Emerfions.				Emerfions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
16	13	45	0*	21	21	9	10	27	4	33	32*
18	8	13	3*	23	15	37	15*	28	23	1	41
20	2	41	9	25	10	5	24*	30	17	29	44*

ECLIPSES of the second Satellite of JUPITER.

Emerfions.				Emerfions.				Immerfions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
JANUARY.				15	22	43	8	15	16	59	43*
				19	12	1	36	19	5	17	23
4	7	4	13*	23	1	22	2	22	18	34	48
7	20	21	36	☉	14	8		26	8	52	26
11	9	39	41	Immerfions.				29	23	9	36
15	22	57	19	APRIL.				JUNE.			
18	26	15	13					2	11	27	0
22	1	33	4	24	9	11	17	6	0	44	27
25	14	51	24	27	22	29	24	9	14	1	41*
29	4	9	33*	MAY.				13	3	54	57
FEBRUARY.								16	16	36	10*
1	17	28	3	1	11	47	54	20	5	53	40
5	6	46	29*	5	1	5	55	23	19	10	55
8	20	4	57	8	14	23	58	27	8	28	19*
12	9	24	50	12	3	41	45	30	21	45	48
								JULY.			

ECLIPSES of the *Second Satellite of JUPITER.*

Immersion.				Immersion.				Emerfions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
JULY.				6	10	29	4*	5	23	30	21
				10	2	58	18	9	12	48	34*
4	11	3	21	13	13	18	8*	13	2	6	15
8	0	20	33	17	2	36	28	16	15	33	40*
11	13	38	46*	20	15	55	57*	20	4	40	52
15	2	56	55	24	5	16	2	23	17	58	3*
18	16	14	40	27	18	34	9	27	7	15	22*
22	5	22	54					30	20	32	11
25	18	51	15	OCTOBER.				DECEMBER.			
29	8	9	39*	1	7	53	17*				
AUGUST.				4	20	52	20				
				8	10	31	19*				
1	21	28	13	11	23	50	14	4	9	38	48*
5	10	46	52*	Emerfions.				7	23	5	35
9	0	6	31					11	12	22	26*
12	13	25	20*					15	1	39	23
16	2	43	20	15	15	41	38*	18	14	56	15*
19	16	2	27*	19	5	0	12	22	4	13	19
23		21	42	22	18	18	42*	25	16	30	23*
26	5	40	56	26	7	36	58*	29	6	47	30*
30	8	0	13*	NOVEMBER.							
SEPTEMBER.											
1	21	19	25	2	10	13	28*				

JANUARY.

ECLIPSES of the third. Satellite of JUPITER.

D.	H.	M.	S.
----	----	----	----

JANUARY.

3	15	59	8	I
	18	43	40	E
10	19	59	35	I
	22	43	5	E
17	23	58	38	I
18	2	42	6	E
25	6	42	35	E *

FEBRUARY.

1	10	42	49	E
8	19	43	54	E
15	18	43	26	E
22	22	49	20	E

APRIL.

21	4	58	50	I
28	8	50	37	I

MAY.

5	12	49	50	I *
12	16	48	53	I

D.	H.	M.	S.
----	----	----	----

19	21	18	22	I
27	1	35	16	I
	3	11	18	E

JUNE.

3	4	42	45	I
	7	7	57	E
10	8	39	52	I
	11	4	38	E
17	12	38	38	I *
	15	2	58	E *
24	16	34	27	I
	18	58	11	E

JULY.

1	20	31	55	I
	22	55	21	E
9	1	30	11	I
	3	52	23	E
16	4	28	18	I
	6	50	16	E
23	8	28	22	I *
	10	48	6	E *

AUGUST.

ECLIPSES of the third Satellite of JUPITER.

D. H. M. S.					D. H. M. S.				
AUGUST.					24	15	36	29	E *
6	16	50	33	I *	31	19	37	27	E
	19	8	50	E	NOVEMBER.				
13	20	52	57	I	7	23	37	36	E
	23	10	59	E	15	3	37	30	E
21	0	56	33	I	22	5	22	40	I *
	3	13	5	E		7	34	22	E
28	4	59	26	I	29	9	24	2	I *
SEPTEMBER.						11	35	22	E *
4	8	53	10	I *	DECEMBER.				
11	13	36	30	I *	6	13	22	28	I *
18	17	9	37	I *		15	33	24	E *
25	21	13	2	I	13	17	20	18	I *
OCTOBER.						19	31	2	E
3	1	16	18	I	20	21	17	21	I
10	5	19	12	I *		23	27	53	E
17	11	29	55	E *	28	1	16	50	I
						3	27	10	E

Now, inasmuch as, in the Beginning of this Year, the Latitude of the fourth Satellite is greater than the Breadth of the Shadow of *Jupiter*, the Satellite will pass wide of it, and there will be no
 2 Eclipse

Eclipse of it till the Middle of *June* in the Year 1752.

It is to be observed, that, for about a Month before, and a Month after the Conjunction of *Jupiter* with the Sun, by reason of the Proximity of *Jupiter* to the Sun, the Eclipses cannot be observed. And this is the Reason that no Notice has been taken of them in the Catalogue between the 24th of *February* and the 25th of *April* following.

The Times here set down are according to the astronomical Way of reckoning, which supposes the Day to commence at the Noon of each Day, or when the Sun is upon the Meridian; and counting the Time on in a successive Order, without the Distinction of Morning and Afternoon, till the Sun returns to the Meridian again the next Day at Noon. Thus, for Example, in the preceding Catalogue, the first Emerfion of the first Satellite is said to happen *January* 1. at 21 Hours 21 Minutes and 11 Seconds; that is, according to the Civil Way of reckoning, on *Jan.* 2. at 30 Minutes 11 Seconds after 9 in the Morning.

An *Addition* to Dr. Hales's Paper, p. 279. by
C. Mortimer.

TWO Days after the Fire-Works had been play'd off in the *Green Park* on account of the late Peace, I went all over the Building erected for that Purpose, and was greatly pleased to see the Doctor's Scheme confirmed by the Practice of the Engineers upon that Occasion; for the Room, in which the Trains were fired; and which was immediately under the Gratings upon which the 6000 Rockers rested and were fired from; had the Floor cover'd over with fine sifted Gravel about an Inch deep, and the Walls were whited over with a dirty sort of white Wash, which I took for Lime finely powder'd, and mix'd up with Size and Water, and done two or three times over. Both Floor and Wall were of Deal.

Printed for C. DAVIS, over-against *Gray's Inn Gate*
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M.DCC.XLVIII.

PHILOSOPHICAL TRANSACTIONS.

For the Month of June, 1748.

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- I. *A Letter from the Rev. Henry Miles D. D. F. R. S. to the President, concerning the Storm of Thunder, which happen'd June 12. 1748.*

S I R,

Read June 23. 1748. I BEG Leave to communicate to you and to the *Royal Society* some Account of the Effects of a Thunder-Storm, on two adjoining Houses, at *Stretham* in *Surrey*, the 12th Instant, a little after 5 in the Morning.

The preceding Day had been remarkably hot, and in the Afternoon very cloudy, with the usual Indications of an approaching Storm, in the Evening. At 9 at Night, the Wind Southerly, my Barometer stood thus; one, which is the most sensible, at 29 Inches $\frac{7}{10}$ $\frac{3}{100}$; the other at 30. $\frac{2}{10}$ 0. The Thermometers (of *Siffon's* Construction) one without-doors, at 43 Degrees, another within, at 49 Degrees above 0. or the freezing Point.

At one next Morning, a Person apprehensive of the Thunder, upon looking out at Window, was surprized to find an unusual clear Sky, every-where equal to what is observed in frosty Weather, or after a high Wind, except that in a few Places some Thunder-Clouds shewed themselves just above the Horizon.

At 2 we heard Thunder at a Distance: At half an Hour past 3, when I got up, I perceived the Storm approaching apace from the South, where the Wind then was, but the darker Clouds seemed to bear off

E e e

chiefly

chiefly to the East and West of us, so that I did not think we should hear of any Mischief near us. At 4 we had a smart Shower of Rain, and about 5 two loud Claps of Thunder over our Heads, but pretty high; the Lightning was very pale, and the Flashes large, descending in a spiral Form, almost perpendicular to the Horizon to the Eastward of us which is the Situation of *Strettham*, and at about 2 Miles distant from us. At a little before my Barometers stood thus, 29. $\frac{7}{10}$ $\frac{3}{100}$, and 30. $\frac{2}{10}$ $\frac{2}{100}$; and continued successively rising and falling during the Storm, but very inconsiderably.

Upon hearing two Houses were damaged, situate at the Foot of the Hill on which the mineral Wells are, fronting the East, by the Wood-Side, I went next Day to view them. The House to the South, which is a public House kept by Mr. *Howard*, seem'd to have received the greatest Shock. Some of the Family being up, the front Door stood partly open, when the Storm began: The upper half was of Glass, fram'd like a Sash-Window, having two sliding Shutters, one on each Side, which had not been taken down. The Glass between them was shatter'd to Pieces, but the Shutters no-ways touch'd, except that a Nail in one of them was forc'd in a little way. To the Door-Post, on the left Hand, hung by an iron Pin an iron Bar, which served to fasten the Door at Night: This Pin was driven out of the Post, and the Bar considerably bent, and in divers Places melted in small Spots, as were the Hinges of the Door, chiefly upon the Edges in both, and the Door-Post split. A Sheet of Lead on the Pediment, or Shelter over the said Door, was raised, and partly rolled
up

up at one Corner; the Cornice underneath being torn off without being split, a good Part of the Tiling near the Eaves and over the Pediment was loosened, and some Tiles beat off, and the Lathing and some of the Moldings of the Windows had taken Fire.

In a Bed-Chamber fronting the Road, on the second Floor where Mr. *Howard* lay, three Boards of the Lining of the Room, on the East Side, were driven inwards five or six Inches at one End; but at the other the Nails were a little loosened only. In a Garret over this Bed-Chamber, the upper Part of a Bed-Post was shiver'd; and nearly over where this Bed stood, a large Hole was broke in the Roof, on the West Side, just by where one of the Chimneys goes up; the Chimneys having all additional Funnels of Brick-Work on the Top, of a roundish Form, and plaster'd: These were struck, and inclin'd to the North, especially that which was on the South End of the House, the Plaster being beat off, and some of the Bricks broke down. There were about 13 Persons in this House, none of which received any Hurt; tho' a Lad, who was in the Kitchen, into which the Door open'd, before-mention'd, and the Window of which (near where he was standing) had several Panes of Glass broke, must certainly be much exposed. He inform'd me, among other things, that the Fire flew about him in Sparks, like those which fly out of burning Charcoal, but larger, and snapping as they do. Some Pieces of Glass were shewed me, which I found to have been melted, one of which I take the Liberty of laying before you.

The adjoining House, inhabited by Mr. *Figgins*, had the Plastering beat off in the Front in Patches, and one of the Chimneys crack'd for a great Length. In the Kitchen Window-Frame, one of the cross Pieces, near the middle of the Window, had a Chip struck off from it about 5 Inches in Length, and at one End about a Quarter of an Inch thick, but thin at the other, and near the Width of the Frame, but none of the Glass broke, nor the Lead bent, tho' in a manner contiguous with the Splinter beat off. The same thing happen'd to a Parlour-Window, on the other End of the House; both the Shivers were found directly opposite to the Windows, at ten or twelve Yards distant in the Road.

In a small Garret (which is next to Mr. *Howard's* House) where two Maid-Servants lay, the Plaster was broken, to Appearance, inwards, on opposite Sides of the Room, and near the Feet of the Bed, which stood on each Side about three Quarters of a Yard from the Wall. The Breach on the East Side, near a Window (some Panes of the Glass of which were broken) was opposite to the Vailings of the Bed, which were singed, and a Hole burnt thro' them big enough to receive the End of one's fore Finger. On the opposite Side, just by the Chimney, another Breach was made, of the same Height; in the Wall, which was continued downwards for about a Yard, but the Curtains not at all singed. Directly against this Breach, one of the Maids (who had got up) sat on the Bed's Side, who was instantly struck down, but received no Hurt: Upon enquiring of her, whether she seem'd to receive a Blow on
any

any particular Part of her Body? she replied, she was struck all over alike.

But the most remarkable, tho' the least terrible Effect, appeared on the Frame of a Pannel of Wainscot, about five Feet long, and about one and a half wide, in the Parlour fronting the East: On this Pannel a Landscape is painted, and the Moulding belonging to it had been gilt, but on the last painting the Room, the Gilding was cover'd with the same Paint: That which cover'd the gilt Moulding was stripped off in irregular ragged Streaks throughout, so that the Gilding appeared as fresh as it may be thought to have look'd when it was painted at first: And as the Gilding does not seem to have been affected, so neither does the Paint appear to have been crack'd any-where, but where the Gilding lay under.

If it be supposed, that the Lead in the Paint was melted by the Lightning, it will be difficult to account for it, that it should *not at all affect* the Paint contiguous with that which was upon the Gilding; tho' we suppose a Resistance to have been made by the Leaf-Gold, and to have contributed to the producing the mention'd Effects. But fearing I have been already too prolix, I ask Leave to subscribe myself, with the greatest Respect,

S I R,

Your and the Royal Society's

Teotling, June 23.

1748.

most humble and most obedient Servant,

H. Miles.

II.

II. *A Letter from John Byrom M. A. F. R. S. to the President, containing some Remarks on Mr. Jeake's Plan for Short-Hand.*

Honoured Sir,

Read June 23.
1748.

AS it has been suggested to me, that I should take some notice of the Plan for a Short-Hand by *S. Jeake* Esquire, which was lately * read before us at the *Royal Society*, I take the Liberty of addressing to you the following Remarks upon it; being obliged to thank you for the public Testimony which you were pleas'd to give, on that Occasion, in favour of the Method which I had the Pleasure of communicating to you; and which, in your Judgment, confirm'd by the Experience of many other Gentlemen who have learn'd it, appears sufficiently to be perfected to Demonstration.

In the Paper read before us it is inferr'd from the continual Succession of new Short-Hands, that none of them were constructed upon right Principles, which, in the Opinion of the Proposer of this Plan, are briefly these;

1. There are in Nature but 8 simple Characters, viz. 4 rectilinear ones, (| — / \ A N) and 4 crooked or semicircular (\cap \cup \supset \subset).

2. To avoid the Ambiguity and Confusion that must arise from the Use of compound Characters, a perfect Short-Hand should consist of these 8 simple ones only.


3. But whereas there are 3 times as many Letters (or more) in the common Alphabet, the Consequence

* See *Phil. Transf.* n. 437. p. 345.

quence is, that one Character must serve for one, two, three, or four Letters; as their Frequency of Occurrence, or Affinity to each other, shall suggest.

4. From these Suppositions, amongst a Variety of Alphabets that would equally answer his Intention, results the following, which (omitting, as needless, the Letters *a, e, i, o, h*) he proposes for the Plan of a perfect Short-Hand; and computes, with great Exactness, that it may be written in less than one Quarter of the Time that common Long-Hand will require.

The Alphabet.


dt. lr. mn. uw. csxz. bfp. egkq. y.

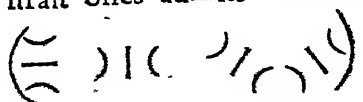
This, Sir, with a Specimen of the Lord's Prayer, as written in it, is the Whole of his Plan; which, as far as it goes, might have a plausible Appearance to a Gentleman, at the first Turn of his Thought towards Short-Hand; but a little practical Attention must have shown him how liable it was to the very Objection that he intended to remedy, *viz.* Ambiguity.

The first Mark, for instance (L), in this short Specimen, stands for these 4 several Words which occur in it, *viz. our, will, evil, ever*; and forty more that one might enumerate, must, whenever they occur, be represented by it; not to mention how often it must occasion Ambiguity in the Beginning, Middle, or End of a longer Word, or Marks, whereof it is a constituent Part only.

Now, though in the Lord's Prayer it is easy, or in casual Writing one of his Learning and Sagacity

city might be able, by a long Familiarity with the Characters, to determine the Sense of what was written in them, yet it is evident, that, to common Learners, a Difficulty so perpetually occurring must appear insuperable.

The Postulatum, likewise, which this Plan for Short-Hand is grounded on, is taken up too expeditiously; for, there being, in Nature, 4 rectilinear Strokes, the horizontal, the perpendicular, and the acute, and grave (if I may so call them); it is manifest, by Inspection, that from these 4 Directions there will arise, at least, 8 curvilinear Characters, as each of the strait ones admits distinctly of 2 oppo-

site Curves  and there is


no absolute Necessity that any of them should be always semicircular; a Shape that, for the most commodious Combination of simple Characters, is in fact much oftener inconvenient than otherwise.

The Alphabet then of simple Characters may be fairly enlarged by one third; and Room be also left for the Fancy of an Inventor to extend it farther, if he should find it convenient upon the Whole.

I say, upon the Whole; for the worst Short-Hand may happen to express a few particular Words better than the best; and arbitrary Marks for Words or Sentences may be often shorter than regular ones: But this is no Inducement to write, in one Case, by a bad Method, and in the other, by none at all.

Another Oversight, in this Plan, is the Neglect of Beauty and Linearity; though the Simplicity of its

its Characters does not, perhaps, admit of such enormous Scrawling as others may.

For, to instance again in the Specimen; suppose the Mark for the Word Temptation  which ex-

presses a vast Variety of different Combinations of Consonants, to be limited, by a previous Knowledge of the Language, to that Word only, yet, after all, it is a very awkward one; and ought, by a common Short-Hand Rule of leaving out such Consonants as are not sounded (as the *p* is not in *Temptation*) to have been form'd in another Manner (*M*) wherein the Beauty and Linearity, and, of course, the Brevity of the Mark would have been preserv'd.

But Emendations of this Nature would, I doubt, in many Cases, which continued writing upon this Hypothesis must exhibit, be utterly impracticable.

In short, this Gentleman set out upon right Principles, which many hap-hazard Undertakers have but little consider'd; but he had not Leisure enough, perhaps, to examine them to the Bottom; as was the Case with Dr. *Green* of *Cambridge* (he that wrote the *Greenian Philosophy*, as he calls it), who form'd a Short-Hand for his own private Use, upon much the same Plan and Principles. He gave me one of his Sermons in it; and, upon Suggestion of the Advantages that he might have taken, he said, that for want of Time to consider of his Scheme more thoroughly, when he first adopted it, he had overlook'd them.

A perfect Short-Hand, I suppose, would be a Solution of some such Problem as this: — “A Language being given, to assign the most compendious
- F f f “Method

“Method of expressing it readily, and legibly, by an Alphabet, and Rules, the best adapted to that Purpose.”

How easy soever the general Principles of such a Method may appear to be, there is a deal of Nicety required to put them in Execution : An exact Attention to continual Trials and Amendments is necessary to ascertain the Preference amongst an infinite Variety of Dispositions, which Inventors may select, and throw their Characters into. So that it is no Wonder, that so many Publishers of new Short-Hands content themselves merely with Newness, or at most with some still imperfect Meliorations of foregoing Attempts.

This Gentleman proceeds no farther than to make an Alphabet for his Plan* ; but must be sensible, that, were it never so complete a one, many compendious Applications of it might be obtain'd by a proper Enquiry into the Nature of our Language (the most happily susceptible of this Art of any) and the Abbreviations which it admits of, very intelligibly, in Writing.

And, in his Alphabet, he entirely omits the Letter *b* (which is often wanted), and the Vowels *a*, *e*, *i*, *o*, and yet retains the Vowel *u*, which is certainly as needless as any of the rest : But as a single Point, in five distinct Situations, would have provided for them all alike, he might as well have added that to his Plan, in order to express any particular Vowel, upon Occasion ; because it would
not

* Mr. *Feake* only offers his Plan as the mere Elements of a Short-hand, leaving it to every Practitioner to build upon his Foundation, as they shall judge necessary from Practice : He retains the *u*, because it often stands for *v* or *ve* or *w*. C. M.

not have hurt his Alphabet; and because the Reading of his Short-Hand without any Vowels at all, is so extremely difficult.

For, as one of his strait Strokes (\) must stand for the Words *am, an, in, on, no, me, him, home, &c.* and one of his crooked ones (c) for *as, is, us, so, has, his, ease, ice, use, ax, ox, &c.* and so of the rest; he would himself, in all Probability, be often at a Loss to distinguish what he had written, upon his own Plan *.

The Consonants *j* and *v* he has taken no notice of; as if the common Way of repeating 24 Letters did really give a just Idea of an Alphabet; which it does not; nor can a perfect Short-Hand for our Language (or any other respectively) well be plann'd, without considering the real Alphabet, or Table of every particular Sound, or Modification of Sound; that is to say, Vowel or Consonant which occurs in it; and then adjusting the proper Characters to them, and taking all the Advantages that either Nature or Custom may afford.

I do not recollect, that any Author has ever enter'd into such a Disquisition with a View to Short-Hand; but, with a View to other Advantages, many different Accounts have been given of a real or universal Alphabet; several of them by eminent Members of the *Royal Society*: That propos'd by Mr. *Lodwick*, in particular, is publish'd in the *Transf.* N^o. 182. but might, I think, be reduc'd into less
Compass,

* Vowels may be known to be antecedent or consequent, by the Mark being wrote above or below the Line of Level: e. g. *c as, c sa;*
\ am, \ ma: The Ambiguities in many of these Words are not important, viz. *as, bat, is, his, use, us.* S. 7.

Compass, and set in a plainer Light; and if it be agreeable, I shall, on this Occasion attempt to do it.

In the mean time, Sir, it will not, I hope, be thought impertinent in me to offer these Remarks upon the Plan that has been laid before us, of an Art which I have taken so much Pains to cultivate, and bring to that Perfection which my first and last Intention of introducing one common Standard, for the general Practice of it, requir'd.

If I have succeeded, Gentlemen will, I persuade myself, concur to facilitate the Design; which tho' the Accidents of Life, at a Distance from this Place, have hitherto retarded, I am intent upon accomplishing to the utmost of my Power.

I thank you, Sir, for the many Marks of Friendship which you have shown me, and am

Your obliged humble Servant,

J. Byrom.

III. *Part of two Letters from Mr. B. Cooke F.R.S. to Mr. Peter Collinson F.R.S. concerning the Sparkling of Flanel, and the Hair of Animals in the dark.*

I.

Newport, Isle of Wight, May 19. 1748.

Dear Cousin,

Read June 23. 1748.

I FANCY at last this Sparkling of the Flanel *, and such-like Bodies, will be found to be quite electrical: And it is possible, I conceive

* See *Phil. Transf.* n. 483, p. 457.

conceive, that the acid Steams of the Sulphur, burnt under the extended Flanel in the Time of bleaching, may unite themselves with the Oil (with which Hair, as well as Horns, are found by Analysis to be replete), and form an animal Sulphur, which, upon Friction, Vibration, or any nimble Agitation of these Hairs, may become luminous.

And that something like this may be in the Case seems not improbable; since it hath been observed, that this Appearance hath happened most conspicuous in frosty Weather; in which Season there is generally not only a greater Purity of the Air, and Absence of Moisture; but all hairy and horny Substances (and Hairs you know are but small Horns) are more elastic, and consequently susceptible of, and capable of exciting, the strongest Vibrations. And, on the contrary, the lixivial Salts used in washing may destroy the sulphureous Acid, and discharge the Oil; whence the Hairs will become more flexible and limber, and be rendered less fit for exciting the electrical Fire. And the same may happen when Flanel is much worn, and by that means filled with the alkaline *Effluvia's*, which go off from most (of the higher Order of) Animals by Transpiration; which may dissolve the animal Sulphur, weaken the Spring of the Hairs, and so render the Phenomenon more difficult. I am,

My good Friend,

Most sincerely yours,

Benj. Cooke.

II.

Dear Cousin,

Newport, June 1. 1748.

IT should have been mention'd, that the Flanel had been worn but few Days; and that it was immediately upon shaking the Under-Coat from that which was worn above it, that the Sparks were emitted; and that their Appearance was in a broad Streak almost contiguous, attended with a Crackling or Snapping, like what may be observed on moving the Finger nimbly along over the prime Conductor, when excited in the electrifying Machine; of which the Lady was able to form a Comparison, having afterwards seen some Experiments of that sort.

This Appearance returned at the same time, and on the same Occasion, two or three Nights after, but more languid, till it was quite lost.

A Lady, who was informed of this, lessened the Surprize (which had been thought almost ominous) by assuring, that she had seen the same Phænomenon often in new Flanel, but never in any that had been long worn or wash'd: And that the Flanel being render'd damp with Sea-Water, and afterwards dried, would heighten the Flashing which she imputed to the Sulphur us'd in bleaching. However that be, I shall only observe, that these Sparklings had the crackling Criterion of electrical Fire; and that Hair and Wool, as well as Silk, are Electrics *per se*, and unctuous and sulphurous Bodies more electric than others of the same Density.

Dr. *Wall* hath oblig'd the Public with a curious Dissertation on a similar Subject, which I guess would
be

be particularly entertaining while you are on this Speculation. See *Phil. Trans.* N°. 314, p. 69.

Bartholin supposes unctuous *Effluvia* to have a great Share in these Appearances: His Words are these, which I chuse to quote; the Book * *de Luce Animalium* being not very common: “ Imo quod
“ admirationem excedit, collectæ oleaginosi effluvi
“ reliquæ, longo interjecto tempore, in scintillas
“ resolvuntur: si enim fascias vel tænias serico textas,
“ sed usu detritas, leviter excutiamus, igniculi susci-
“ tantur et scintillæ:”--- and quotes a Passage out of *Gesner de Herbis lucentibus*, to confirm his Opinion.

The same Writer tells us, that *Theodore Beza* was to be seen in the dark, “ ob fulgorem externum
“ circa oculorum orbes;” — but whether this Light proceeded from the Ball of the Eyes, or Hairs of the Brows or Lids, he does not mention. — Nor is that learned Author so exact in some other Circumstances in other Examples of this sort as could be wish’d. However, I think what he says of the Duke of *Mantua* deserves a Remark. — “ Quicquid
“ sit, pro vero habendum est quod de *Carolo Gen-
“ zaga Mantuæ* Duce constans fama tulit, levi per
“ totam cutem facta *frictione* flagrant species exire
“ solitas.” — But here also it were to be wish’d he had let us know whether this great Man, of a most illustrious Family, had not some particular hairy or scaly Texture or Covering to his Skin.

By this, I guess, you are excited to know how this Author, who liv’d about a hundred Years past,
solves

* *Tho. Bartholinus de Luce Hominum et Brutorum, lib. iii. Hafnia 1669, 8º.*

solves these Appearances, of which he had professedly wrote. Take it in his own Words.—

“ *Aristoteles* (l. i. m. m. cx.) docebat — quod
 “ omnis natura ejus sit essentiæ procreatrix, qualis
 “ ipsa est — enimvero sunt ad conservationem speciei
 “ omnis, ejusdem singulæ particulæ, vim se diffundendi
 “ obtinuerunt, et spargendi, per individua multiplicata,
 “ ita ne lux primæva et naturalis, singulari numinis
 “ consilio, elementorum mixtioni addita, mole minor
 “ intercidat, et extingatur cum speciei non revocando casu,
 “ eo modo conservari debuit, quo servantur omnia,
 “ per insitam naturæ potentiam sui generativam, &c.”

IV. *A Letter from the Reverend Mr. John Forster to Mr. Henry Baker F. R. S. concerning an Earthquake at Taunton.*

S I R,

Read June 15. 1748. **I**N Answer to your Inquiries concerning the Earthquake, which happen'd last Year on the first Day of *July*, when I was at *Taunton* in *Somersetshire*, after taking some Pains to inform myself more particularly what other People observed in different Places, you may depend on the Truth of what follows.

Between Ten and Eleven o' Clock at Night, on the said first Day of *July* 1747. being myself in some Company at *Taunton*, we were suddenly surpris'd with a rumbling Noise like distant Thunder, which was followed immediately by so considerable a Motion

Motion of the Earth, that the Chair whereon I sat rocked under me. The Noise and Shaking seemed to come from a Distance, and approached gradually, in such a manner as if a loaded Waggon had passed along; and continued nearly the same Time as such a Waggon would require to go about an hundred Yards. The Motion went from South-East to North-West; which being the Direction of the Street, on one Side whereof the House stood, some of us imagined at first that a Waggon had really gone along*; but, upon running out and enquiring, we found there had been no Waggon: And indeed, as we were satisfied afterwards, no Waggon could have been heard or felt in the back Room where we sat, on account of its too great Distance from the Street.

Norwithstanding this happened between Ten and Eleven o' Clock at Night, when most of the Town were in Bed, the Shock was so sensible, that many People got up very much terrified; and they waking others, the Consternation soon became general; in-somuch that, altho' it was a rainy Night, Numbers of People ran out into their Gardens, and spent the Night there, being apprehensive of other Shocks. The Account then newly brought us of a dreadful Earthquake at *Lima*, being fresh in every body's Mind, contributed to increase the Surprise.

A worthy Clergyman, who lives five Miles from *Taunton*, informed me, that the China and Glasses upon the Cupboards in his House rattled and shook as if they would fall down, and the Bells in his
House

* See something like this in *Phil. Transf.* n. 455, p. 289.

House rang. A Person who was at that time coming on foot to *Taunton* likewise told me, that the Noise seemed to him like the Discharge of Cannon at a Distance, and came rumbling onwards, till the Earth moved under him in such a manner that he could hardly keep upon his Legs: Several others also that were abroad assured me they had much ado to save themselves from falling.

The Extent of this Earthquake, as far as I can learn, was from Sea to Sea; that is, from the *South Chanel* to the *Severn*. It moved from South-East to North-West, and was felt in every Parish through this whole Course, which is in Length about forty Miles: Nor was its Breadth much less; for it was felt at the same time both at *Exeter* and *Crookhorn*, which lie from one another about the same Distance of forty Miles, in a Line directly across its before-mention'd Course.

This, Sir, is the best Account I am able to give; and I shall add nothing more, but that I am

York-Buildings, June 28.
1748.

Your most humble Servant,

John Forster.

P. S. I have heard it reported that there were Flashes of Lightning at the time of the Earthquake; but I neither saw any myself, nor have met with any body that could affirm he did.

V. *A Letter from John Byrom M. A. and F. R. S. to the President, containing some Remarks on Mr. Lodwick's Alphabet.*

Honoured Sir,

Read June 30.
1748.

HAVING, by your Permission, borrow'd N^o. 182. of the *Transactions*, wherein is contained *An Essay towards an Universal Alphabet, by Mr. Francis Lodwick F.R.S.* I shall give a brief Account of it; and, in Obedience to your Commands at the last Meeting, endeavour to shew how it may be reduced into less Compass, and set in a plainer Light.

Mr. *Lodwick* premises first the Advantages of such an Alphabet; which I may as well refer to as repeat; they all center in acquiring, describing, or perpetuating the true Sounds of any Language, by a Standard Character for all.

He then defines what a single Sound, what a compounded one, a Vowel, Consonant, Diphthong, and Triphthong is; all which is likewise sufficiently obvious, and needs no Repetition.

To proceed directly, therefore, to his Alphabet, or Collection of all the single Vowels or Consonants which are used in any Language, the Number of Vowels is, according to him, 14, which are, all but 3, expressed in *English* Words in the following Table.

- | | |
|------------------------------------|--|
| 1. <i>a</i> — <i>tall</i> | 8. <i>ui</i> — <i>muis</i> . Low Dutch |
| 2. <i>a</i> — <i>tallow</i> | 9. <i>y</i> — <i>tile</i> |
| 3. <i>a</i> — <i>tale</i> | 10. <i>o</i> — <i>tone</i> |
| 4. <i>e</i> — <i>tell</i> | 11. <i>u</i> — <i>tunne</i> |
| 5. <i>ea</i> — <i>teal</i> | 12. <i>u</i> — <i>une</i> . French |
| 6. <i>i</i> — <i>till</i> | 13. <i>oo</i> — <i>tool</i> |
| 7. <i>u</i> — <i>dure</i> . French | 14. <i>ou</i> — <i>tould</i> . |

These, he says, are the Vowels, *each of which is long, and short; short, as in the Words God, Man, Sin; long, as in Ball, Demand, Seen, &c.*

As any Vowel sounded by itself is naturally long, I take it for granted that he intended the 14 in this Table to be all such; and yet, in the Words *Tallow, tell, till, tunne*, the Vowels, as we now pronounce them at least, are all short; and in the Words *tile* and *tould*, a Diphthong (or Composition of 2 Vowels pronounc'd in the Time of one) is sounded: So that there are but 5 long Vowels accounted for in our Language by the Words *tall, tale, teal, tone, tool*. The foreign Words, as it would be nice and endless to dispute about, so it comes not within the Compass of my present Design; which is to give a List of Vowels, whereby to discriminate, as conveniently as may be, all the Instances of Vocality that occur, distinctly, in the *English* Language; for which I apprehend that half the Number in his Catalogue, or 7 Characters, would be, to all attainable Purposes, sufficient.

As we commonly reckon but 5 Vowels in our Alphabet, *a, e, i, o, u*; two of which, *viz. i* and *u*, are really Diphthongs, I must denote the single Vowels

Vowels by 2 apiece, as Custom sometimes does, to shew which I mean; and, beginning from the smallest Aperture of the Organs to the most dilated, they are these.

1.	<i>oo</i>	<i>ooze</i>	<i>fool</i>	<i>too</i>
2.	<i>oa</i>	<i>oat</i>	<i>stole</i>	<i>to</i>
3.	<i>ee</i>	<i>eel</i>	<i>steel</i>	<i>see</i>
4.	<i>ea</i>	<i>eat</i>	<i>steal</i>	<i>sea</i>
5.	<i>ai</i>	<i>aid</i>	<i>stale</i>	<i>say</i>
6.	<i>aa</i>	<i>aaron</i>	<i>stamen</i>	(sol)fa
7.	<i>au</i>	<i>autumn</i>	<i>stall</i>	<i>saw</i>

Though vocal Sounds, like instrumental, may, in Speculation, admit of numberless Distinctions, y.^et, as Experience shews that 7 Notes, flatten'd or sharpened, upon Occasion, suffice for a practical Gamut, or Scale of Music, so I incline to think, that 7 vocal Notes or Vowels, varied in some correspondent manner, or struck, as one may say, in diphthongal or triphthongal Chords with each other, may well enough account for the Sounds of our Language; or possibly of any other, if it be consider'd, that different Voices, as well as Instruments, have somewhat so peculiar in them, that nothing but the Ear itself is able to distinguish.

However, as far as these 7 Vowels extend, if they were denoted by any common Characters, as, suppose at present, by the 7 first numeral Figures, the absurd Variety which Custom has introduc'd of expressing the same vocal Sounds, amongst different Nations, even using the same alphabetical Characters,

ters, would, in a great measure, be immediately corrected.

One Instance may serve for many; the Word *we* in *English*, and the Word *oui* in *French*, however differently written, have the same Sound, or Composition of Sound, from our Vowels *oo*, and *ee*, or their *ou* and *i*; if then the Figure 1 was always to denote the Sound that we express by *oo*, the *French* by *ou*, the *Italians* by *u*, &c. and the Figure 3 was to denote our *ee*, and their *i*, in like manner, the combin'd Character, or Diphthong 13, would be founded alike by all Readers of any Nation, who should previously be agreed upon such a common Character.

And allowing Mr. *Lodwick's* Notion, that there are 3 Vowels in other Languages, which ours has not, there will remain the Figures 8, 9, 0, to express them by — or any other Characters may be pitch'd upon. What I aim at is to shew, that, through an over Pursuit of Accuracy, he has multiplied his Vowels, without any apparent Necessity.

And the Case is the same with respect to his Consonants, which he thus ranges into 11 Files, and 6 Ranks.

1.	2.	3.	4.	5.
1 B <i>bond</i>	D <i>dark</i>	J <i>jest</i>	G <i>game</i>	=
2 P <i>pond</i>	T <i>tart</i>	Ch <i>chest</i>	K <i>came</i>	=
3 M <i>mind</i>	N <i>name</i>	gn <i>seign</i>	Fr. ng <i>song</i>	=
4 =	dh <i>this</i>	j <i>jean</i>	Fr. g <i>gaen</i>	} Low V <i>Valley</i> Dutch F <i>Folly</i>
5 =	th <i>thing</i>	sh <i>shall</i>	ch <i>dach</i>	
6	n <i>danse</i>	French.		

6.	7.	8.	9.	10.	11.
=	L lane	H band	Y yarn	R rand	W wand
=					
=					
Z	zeal	lh	Welsh		
S	seal				

By this Distribution one is led to think, at first, that he would hint that there were in Language 11 times 6, or 66 Consonants; though Experience had taught him to complete the 6 Ranks of his second File only.

But, as the Mark (=) by which he signifies that there are indeed analogous Consonants that might be express'd in those Places, but with a *Difference that would be too nice for common Discernment; and such as he had never heard expressed in any Language*; as this Mark, I say, occurs but 8 times, I will suppose him to take the real Number of Consonants in Nature to be 37, whereof 29 may answer the Purposes of an universal Alphabet.

To reduce both Files and Ranks into less Compass, and plainer Order, I would take the real Consonants of his first and fifth Files into one File or Rank; and place them in this manner

P. B. M. F. V.

dismissing the 5 unexpress'd (and, for any thing that appears, inexpressible ones) as imaginary.

And to this Order of Consonants all such as are used in our Language may be adjusted; for, beginning thus with the labial and labio dental Consonants, and so proceeding to the Gutturals, they will stand, in Rank and File, after this manner.

P. B. M. F. V.
 S. Z. R. Sh. Zh.
 T. D. L. Th. Dh.
 K. G. N. Ch. J.

wherein their mutual Analogy and Correspondence seems to appear in the plainest Light that one can possibly put them in ; as a little Attention will better discover, than a Proximity of Particulars explain.

If not, I shall be glad to be set right, if I am mistaken in supposing, 1. That here is no Consonant omitted, which is really used in our common Pronunciation ; or, 2. That here are none superfluous, or compounded ; or, 3. That, in this View of them, their Relation to each other is the most discernible.

I except the Letter *H*, which may have its Place amongst the guttural consonantal Aspirates, which some foreign Nations are accustomed to, but ours, in general, is not: And these, as I conceive, will not be found to be distinctly more than what the Addition of a fifth Rank to the foregoing may exhaust.

The Power or Force of this peculiar Letter *h* is so capable of Intermixture with that of others in this Table, and that of the Naso-guttural *N*, of sliding, without its full Expression, into a following Consonant (as it does particularly in *French* Pronunciation); that they have led the Writers on this Subject to imagine Consonants in Nature, which they endeavour to express by *N French*, *gn*, *ng*, and by divers Changes of the Letters *h*, *n*, *g*, that give an attentive Examiner no clear Idea of any distinct Consonant, but rather
 perplex

perplex the Matter by unintelligible Niceties, and hinder the Prospect of an universal Alphabet, by seeming Impossibilities of arriving at it.

I and *W*, which constitute Mr. *Lodwick's* 9th and 11th File of Consonants, are equal to the Vowels *ee* and *oo*: His two *Dutch* Consonants may have their Place in the 5th Rank above-mention'd.

So that omitting such of his 29 as ought, if these Observations are just, to be omitted, there will remain the 20 that are here digested into 5 Files and 4 Ranks, for the real Alphabet of Consonants in the *English* Language; together with the *H*, which whether it may be reckon'd one or not, I shall leave to the Grammarians.

The 5 Files, for an Assistance to the Memory, may by the Help of intermediate Vowels, be compris'd in 5 technical Words of an hexameter Verse, such as,

PaSTiKa, BoZDaGo, MeRLiN, FiShThoChe, VeZhaDhJo.

And the 4 Ranks, as they begin with the Letters *P, S, T, K*, may, for the same Reason, be called *Prima, Secunda, Tertia, Quarta*, (= *Kuarta*) Classis.

These 20 Consonants may appear to be too few; but I much question whether the real Alphabet of any Language has either more in Number, or better fitted for the Purposes of Speech than this of ours: The Difference lies chiefly in the Gutturals, which the *Orientals, Welsh, Germans*, &c. pronounce differently from us.

Our Neighbours the *French* (which is very remarkable) few as these 20 are, have but 16 of

H h h

them;

them ; and yet they are able to express themselves with greater Fluency and Precision than we can well be Masters of, till we shall imitate their Care to polish and to propagate their Language, by some Attention to the Improvement of our own.

They have none of our *th*, *dh*, *ch*, or *j* ; and if a Man's Name, suppose, were *Thatchth' edge*, they would not be able, without previous Practice, to pronounce any one of these 4 Consonants, which help to compose it, and which Custom obliges us to denote, so absurdly, by 9 Letters that have not the alphabetical Force of any one of these 4 amongst them.

The most important Reflection upon the Subject is this, that whereas we have in our Language but 7 distinct Sounds or Vowels, and thrice the Number of Stops or Modifications of them ; if we had accordingly 28 Letters or Types appropriated to them, and always wrote or printed what we spoke, the Theory of Reading might be acquired in as few Hours, as it costs at present Months or Years to acquire it in.

But I forbear the Pursuit of this Topic any farther ; understanding, from Gentlemen who were desirous that I should examine Mr. *Lodwick's* Scheme immediately, that the *Society* would break up for the Summer, at the next Meeting : Attendance upon my Short-hand Scholars has obliged me to urge what occur'd upon the Perusal of it as briefly as I could, and so I submit it to Consideration, and am, &c.

VI. *A Roman Inscription found at Bath, communicated to the Royal Society by the Rev. William Stukely M. D. Fellow of the Coll. of Phys. F. R. S. and Rector of St. George the Martyr, London.*

Read June 30.
1748.

L ▽ VITELLIVS ▽ MAXIMI
NIANI ▽ F ▽ T ▽ ANCINVS ▽
CIVES ▽ HISP ▽ CAURIENSIS
EQ ▽ ALAE VETTONVM ▽ CR
ANN ▽ XXXXVI ▽ STIP ▽ XXVI ▽
H ▽ S ▽ L ▽

Thus to be read. *Lucius Vitellius Maximiniani filius Titus Ancinus, * civis Hispanus Cauriensis & equitum ala Vettonum curator, annos XLVI. stipendii XXVI. hic sepultus est.*

VII. *Extract of a Letter from the Rev. Dr. Stephen Hales F. R. S. to the Rev. Mr. Westly Hall, concerning some Electrical Experiments.*

Dear Sir,

Teddington, Feb. 23, 1746-7.

Read June 30.
1748.

THE Favour of yours of Jan. 27. I should have answer'd sooner, but have been prevented by Variety of Business, especially

H h h 2

* Like *nubes, labes, sepes.*

§ Of the City of Coria in Spain.

cially the being much in *London*; where I saw last Week some electrical Experiments; in which new Field of Researches there are daily new Discoveries made: The active electric Fluid seems to be a great Agent, in Conjunction with the Air, in the Production of Fire.

A warm thick Piece of Iron being suspended by two silk Lines, had a warm very thick Piece of Brass laid on it, on which was placed a common Hen's Egg: When electrified, the Flashes from the Iron were of a bright silver light Colour; from the Brass (especially near it) the Flashes were green; and from the Egg of a yellowish Flame Colour; which seems to argue, that some Particles of those different Bodies were carried off in the Flashes, whence these different Colours were exhibited.

It is suspected that great Degrees of electrifying have occasion'd some Women to miscarry; and no Wonder that such sudden Shocks should do it. I wrote to Mr. *King* the Experimenter to electrify a Frog, while the Circulation of its Blood was viewed with a Microscope, to see if it accelerated its Motion, which he has not yet done.

He observes, that a Piece of Linen that has never been washed, will soon give a good Degree of Electricity to a large warm glass Tube; viz. on account of the mealy Paste, which Weavers dress the Linen with; and therefore any Piece of Linen thus dressed will do.

I gave an Account in the *General Evening Post* of *September* last of the great Benefit of Ventilators in *Newgate*, and in the *Success* Frigate for *Georgia*, which lay five Months wind-bound in our Chanel with the Transports for *Cape Breton*, the rest of which were
all

all very sickly ; but in the *Georgia* Frigate, in which were about 300 Men, all were in good Health ; and last Week I was informed that they got all in Health to *Georgia*.

* * *

Your obliged and

affectionate humble Servant,

Stephen Hales.

VIII. *Extract of a Letter from Tho. Aery M. D. to Cromwell Mortimer M. D. Secret. R. S. containing the Particulars of the Cure of a Wound in the Cornea, and a Laceration of the Uvea in the Eye of a Woman.*

Whitehaven, June 14. 1748.

S I R,

*Read June 30.
1748.*

YOUR laudable Endeavours to promote the Art of Medicine, make me willing to hope you will pardon the Freedom I take of acquainting you with the following Case, tho' I have not the Happiness of your Acquaintance ; and I beg the Favour of you to lay it before the *Royal Society*, to publish, if you approve of it ; which will oblige, Sir,

Your humble Servant,

Thomas Aery.
A

A POOR Widow, aged 26 Years, of a pale Complexion, was for several Years now-and-then subject to the Colic. Dec. 26. 1744. she received a Wound in the *Cornea* of her right Eye, by the Spear of a common Fork, which also divided the *Uvea*. Part of the aqueous Humour was discharg'd, the Eye lost its Transparency, had a violent Pain in it, and she could only distinguish Objects when she look'd down. I order'd her a *Collyrium* prepared of the *Bals. Tolut. Camphor. solut. in Sp. Vin. Aq. Plantag. cum pauxillo Tinct. Mart. Mynsiekt.* A few Drops of this blood-warm was to be us'd frequently; to bleed her largely in the Arm, and her Diet was to consist of Water-Gruel, *Aq. Hord.* and fresh Broth.

Next Day she had no Pain in the Eye, but complain'd she saw Motes floating before it: I order'd her a Purge of *Infus. Sennæ*, and an astringent Fomentation to her Temples and Eyelids. The Day following the Eye was inflamed, and the Lids tumefied, and she had a Pain in her Head. The *Collyrium* was changed to Rose-water and Vinegar, aa. ʒ ss. Roche-Alum gr. v. 3 Drops twice a Day. The 29th the Inflammation increasing, the *Infus. Sennæ* and Bleeding were repeated, and the Parts were fomented only with Spirit of Wine. The 31st the Inflammation continued to decrease, till after a Fright. Jan. 5. the Inflammation increasing, the Sides of the Wound became a little protuberant. The *Senna* was repeated, and a Blister laid behind the right Ear, and an emollient *Collyrium* was us'd:

Next

Next Day the Swelling of the Eyelids was gone: The 11th she had a Shew of the *Menses*, and the Wound appeared healed: From the 15th to the 24th the Inflammation continued to abate; only one Day it increased by fretting and weeping much; but by bleeding she grew better; and so she continued to the 30th; unless one Day, upon catching Cold, her Eye became exceedingly inflamed, which was relieved by bleeding. Feb. 4. she had a little Pain in her Eye, and the *Tunica adnata* looked a little red. Soon after dropping in of 2 Drops of cold Water, the Eyelids swelled, and a violent Inflammation of the Eye ensued, with a Speck appearing; but these Symptoms went off by repeated Applications of Leeches and a mercurial Purge. The 19th a Sternutatory of Hellebore and *Euphorbium* was order'd.

In a few Days after the Inflammation left her Eye; when she complained she saw double; which Complaints also soon left her:

The Eye is myopical, and she sees the right Side of Objects a little darkened; yet she can read pretty small Characters. The *Uvea* is not united where it was divided, but still retains its natural Power of Contraction; the Transparency of the Humours and Convexity of the *Cornea* are the same as before; there is no Scar upon the *Cornea*; the Shape of the *Pupil* is much alter'd, as may be seen by the Figure of the Eye, which I send herewith. See Fig. p. 415.

Upon catching Cold she is subject to a slight Pain in her Eye. At present there remains no other Alteration than what I have just mention'd, and what necessarily follows from the Contraction of the *Pupil*, the not admitting a sufficient Quantity of Rays
to

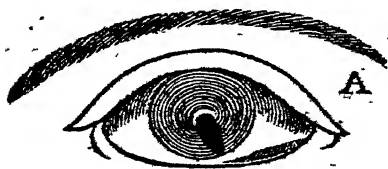
to pass to the *Retina*, upon which Account she is short-sighted. Her seeing Objects darkened on one Side, may proceed from the artificial Part of the *Pupil* being situate nigher to the great *Cantbus* of the Eye than usual in Nature; by which the Rays which fall on the Side of the *Cornea* next to the little *Cantbus* of the Eye, being partly intercepted, must occasion a Defect in the Picture; from which Defect a Darkness will be seen on one Side of the Object. To the Weakness of the Vessels of the Eye we may attribute the Pain of the Eye upon catching Cold: It oft-times happens to those who have had a severe *Ophthalmia*, that, during Life, the small Vessels are too weak; and hence, from slight Causes being distended, they will be painful, and frequently red.

This Case is known to several in this Place, particularly to Mr. *Blencowe*, an ingenious Surgeon and Apothecary.

Some Remarks occur from this Case; *viz.*

1. Her Cure would have been performed sooner, if her Circumstances had allowed of her observing an exact Regimen.
2. When her Eye had little or no Appearance of Inflammation, I tried cold Water, but with rather bad Success. All cold Applications to inflamed Eyes, Astringents or Repellents, require the utmost Caution in applying them; for if they produce not a good, they will produce a bad Effect. In slight Cases they oft have very happy Effects, but where the obstruent Matter is so fixt that it will not suffer itself to be easily repell'd back, the Vessels being straitened, the Fluids coagulated
the

- the Disease will be increased; which happen'd in this Case from the Application of cold Water.
3. The good Effects of Evacuations are very evident in abating the Inflammation. Wounds in the *Cornea*, attended with a Wound of the *Uvea*, and a troublesome Ophthalmy, heal without any Scar.
 5. An artificial *Pupil*, made by slitting the *Uvea* in a different Method from that invented by the ingenious Mr. *Chefelden*.
 6. The Necessity of changing the Topics, according to the State of the Distemper; which has been remarked before by several celebrated Authors.



A, This Figure represents the Eye as it appears at present.

The SOCIETY adjourned to October. 27. 1748.

IX. *Tables of specific Gravities, extracted from various Authors, with some observations upon the same; communicated in a Letter to Martin Folkes Esq; President of the Royal Society, by Richard Davies M.D.*

Presented Feb. 18.
1747.

THE manifold applications which may be made, for the purposes of Natural Philosophy, of the relations which Bodies bear to each other, by their respective Specific Gravities, engaged me some years since to collect all the experiments of this sort I could meet with in the course of my studies, and also to make several new ones of my own with the same design.

When my collection began to be somewhat considerable, I disposed the several bodies in Tables according to their species, which I found to be the most convenient method, as my tables were by this means capable of receiving additions in any part, without destroying the form of the whole: and as they were thereby easy and ready to be consulted, and well disposed for the forming of immediate comparisons between the several bodies of the same species.

But having now no farther opportunities of enlarging my collection, I hereby beg leave to recommend the prosecution of my design to others, as a subject well deserving the attention of some of the members of the *Royal Society*, to whom I therefore present these my tables: wishing they may prove of some

some use and service to the inquisitive and philosophical part of the world. As I persuade myself they really will, when they shall be further rectified, by the omission of the erroneous or uncertain experiments; when they shall be enlarged by the addition of such others, as may still be found in good authors, or which yet remain unpublished in the closets of the curious: and especially if some such gentlemen as have skill, leisure, and opportunities, shall please to supply their remaining defects, by the communication of their own observations, made upon those bodies, whose specific gravities have not as yet been carefully recorded.

*Denique cur alias alijs præstare videmus
Pondere res rebus, nihilo majore figura?
Nam, si tantundem est in Lænæ glomere, quantum
Corporis in Plumbo sit, tantundum pendere par est.*
Lucret.

A short account of the Authors, from whose writings and experiments, the following Tables have been collected, with some remarks upon the experiments themselves, and the manner in which they appear to have been made.

THE antients have left but few particulars concerning the different specific gravities of bodies, tho' it is plain they were in the general sufficiently acquainted with them. It was by the knowledge of the various weights of gold and silver, that *Archimedes* is recorded to have detected the famous fraud committed in *Hiero's* crown, as *Vitruvius* has at large related in his *Architecture*, l. ix. c. 13. and it is from the same great philosopher, that we have derived the demonstration of those hydrostatical rules, by which the proportions are best to be known, of the several weights or densities of different bodies, having the same bulk or magnitude: as may be seen in his tract *De insidentibus humido*, lost in the *Greek* original, but retrieved in great measure, as it is said, from an *Arabic* translation. It was published in *Latin*, with a commentary by *Federicus Commandinus* at *Bononia* 1565, 4°, and the substance of it by *Dr. Barrow* in his *Archimedes*, printed likewise in 4° at *London* 1675.

Pliny, in the xviii. book of his *Natural History*, has set down the proportional weights of some sorts of grain, among which he says that barley is the lightest. *Levisimum ex his hordeum, raro excedit, [in singulos nimirum modios] xv libras, et faba xxii. Ponderosius*

Ponderosus far, magisque etiamnum triticum. And a little further on, *ex his generibus [frumenti scilicet] quæ Romam invehuntur, levissimum est Gallicum, atque e Chersoneso advectum: quippe non excedunt in modium vicenas libras, si quis granum ipsum ponderet. Adjicit Sardum selibras, Alexandrinum et trientes: hoc et Siculi pondus. Bæoticum totam libram addit: Africum et dodrantes. In Transpadanâ Italiâ scio vicenas quinas libras farris modios pendere: circa Clusium et senas.* And the same author in his xxxiii. book, speaking of quicksilver, observes that it is the heaviest of all substances, gold only excepted. *Omnia ei innatant, præter aurum: id unum ad se trahit.* Which *Vitruvius* had also taken notice of, and had mentioned besides the weight of a known measure of it, that of four Roman *Sextarii*. *Eæ autem [guttæ nempe argenti vivi quæ inter se congruunt et una confunduntur] cum sint quatuor sextariorum mensuræ, cum expenduntur, inveniuntur esse pondo centum. Cum in aliquo vase est confusum, si supra id lapidis centenarii pondus imponitur, natat in summo: neque eum liquorem potest onere suo premere, nec elidere, nec dissipare: centenario sublato, si ibi auri scrupulum imponatur, non natabit, sed ad imum per se deprimetur, Ita non amplitudine ponderis, sed genere singularum rerum gravitatem esse, non est negandum.* *Archit.* l. vii. c. 8.

Again, *Q. Rhemnius Fannius Palemon*, in his fragment *De ponderibus et mensuris*, has given us an observation, of the proportional gravities of Water, Oil, and Honey.

—Librae

— *Libræ, ut memorant, bessem sextarius addet,
Seu pueros pendas latices, seu dona Lyæi,
Addunt semissem Libræ labentis Olivi,
Selibramque ferunt mellis superesse bilibri.*

That is to say, that the *Sextarius* of either water or wine weighed 20 ounces, the same measure of oil 18, and of honey 30. Their specific weights were therefore in proportion as 1.0, 0.9, and 1.5, exactly agreeable to what *Villalpandus* determined about the beginning of the last century: Yet was this author himself sensible that these were not to be look'd upon as very nice experiments;

*Hæc tamen assensu facili sunt credita nobis.
Namque nec errantes undis labentibus amnes,
Nec mersi puteis latices, aut fonte perenni
Manantes, par pondus habent: non denique vina,
Quæ campi aut colles nuperve aut ante tulere,
Quod tibi mechanica promptum est depromere:
Musa.*

After which he proceeds to describe a good pretty instrument for the ready finding of the different specific gravities of fluids, and shews how those of solids also may be hydrostatically discovered. And so much shall suffice for what I had to mention from the antients relating to this subject: I now come to those who have written within these last hundred and fifty years.

Francis

Francis Bacon, Lord Verulam &c. in his *Historia densi et rari*, printed in the second volume of his works in folio, London 1741. p. 69. has given a table, which he calls, *Tabula coitionis et expansionis materiae per spatia in tangibilibus (quæ scilicet dotantur pondere) cum supputatione rationum in corporibus diversis*. This tract does not appear to have been published till after his death, which happened in the year 1626, but was probably written several years before; and the experiments were even as he tells us made long before that. *Hanc Tabulam multis abhinc annis confeci, atque ut memini, bona usus diligentia*. I therefore apprehend it to be the oldest table of Specific Gravities now extant. The experiments therein mentioned were not made hydrostatically, but with a cube of an ounce weight of pure Gold, as he says, to which he caused cubes of other materials to be made equal in size: as he did also two hollow ones of silver, and of equal weights, the one to be weighed empty, and the other filled with such liquid as he wanted to examine. He was himself sensible that his experiments of this sort were, notwithstanding his care, very defective, *possit proculdubio tabula multo exactior componi, videlicet tum ex pluribus, tum ex ampliore mensura: id quod ad exactas rationes plurimum facit, et omnino paranda est, cum res sit ex fundamentalibus*. From among these, notwithstanding their imperfection, as they appear to have been some of the first experiments of the sort regularly digested, and as they were besides made by so great a man, I have extracted the specific gravities of the fixed metals, which I have inserted as examples in the following tables: after reducing them to the common

form, upon the supposition that pure gold was, according to *Ghetaldus*, just 19 times as heavy as water. And this I have rather chosen to do, than to make use of his Lordship's own weight of water given in the table, which in the manner he took it could not be very exact, and which besides would not have brought out the specific gravity of pure gold more than 18 times as much; and that of the other metals in proportion. This table contains in all 78 articles.

There are also in the third volume of the same edition of his works, *p. 223*, *Certain experiments made by the Lord Bacon about weight in air and water*. These are truly *hydrostatical*, but very imperfect, I have not therefore inserted any of them in the following collection.

Marinus Ghetaldus, a nobleman of *Ragusa*, published in *quarto* at *Rome*, in 1603, his treatise entitled, *Promotus Archimedes, seu de variis corporum generibus gravitate et magnitudine comparatis*, wherein he has given a comparison between the specific gravities of water and eleven other different substances, from his own hydrostatical experiments made with care and exactness. These I have inserted: expressing the numbers as they stand in his own book, but I have afterwards also for uniformity reduced them to the decimal form. I have besides at the end transcribed at large the two tables of this author, in which every one of the twelve sorts of bodies he treats about is successively compared with all the others, both in weight and magnitude.

Father

Father *Johannes Baptista Villalpandus*, a Jesuit of *Cordoua* in *Spain*, in his *Apparatus Urbis-et Templi Hierosolymitani*, printed in *folio* at *Rome* in 1604, exhibited a table of the proportional weights of the seven metals and some other substances, from his own experiments, made with great care as he tells us, by the means of six equal solid cubes of the fixed metals, and a hollow cubical vessel 8 times as large, for the comparing Mercury, Honey, Water, and Oil with the same. His numbers, which are inserted under his name in the following tables, were also again published afterwards by *Joh. Henr. Alstedius* in his *Encyclopædia universa*, printed in 2 vols. in *folio* at *Herborn* 1630, and by *Henry Van Etten*, in his *Mathematical recreations*, from whence they have been often transcribed into other books. *Villalpandus's* book, which is only the third volume of a work begun to be published several years before, was itself printed so soon after *Ghetaldus's*, that it is probable he either never saw that author, or not at least till after his own experiments were made.

Mr. *Edmund Gunter*, in his *Description and Use of the Sector*, printed after his death by Mr. *Samuel Foster* in 1626, having occasion to make mention of the specific weights of the several fixed metals, quoted *Ghetaldus*, and made use of his proportions, and so did also Mr. *William Oughtred*, in his *Circles of Proportion*, first published in *quarto* 1633, with this only difference, as to the form, that he changed *Ghetaldus's* unit into 210, whereby he expressed all his relations in whole numbers. It is likewise probable that *D. Henrion* took from the

same place the numbers he applied in his *Usage du Compas de Proportion*, printed at *Paris* in 1631, 8°. although he has not given them all with exactness, for the sake as it seems of using simpler vulgar fractions.

Father *Marinus Mersennus*, a *French* Minim, in his *Cogitata Physico-Mathematica*, printed at *Paris* in 1644. 4°, has given from the observations of his accurate friend *Petrus Petitus*; a table of the specific gravities of the metals and some other bodies, making Gold 100, Water $5\frac{1}{3}$, and the rest in proportion. These I have reduced to the common form, and inserted under his name in the following tables. The same were afterwards made use of by Father *Francis Milliet de Chales*, Jesuit, in his *Cursus Mathematicus*, Monsieur *Ozanam*, Professor *Wolffius*, and several others. I have not seen *Petitus's* own book, but it was entitled *L'Usage ou le moyen de pratiquer par une Regle toutes les Operations du Compas de Proportion—augmentées des Tables de la Pesanteur et Grandeur des Metaux &c.* had a privilege dated in 1625. tho' it is said not to have been printed till some years after. The same Father *Mersennus* has also taken notice, in his general preface, of a table of 20 specific gravities, some time before published by Monsr. *Aleau*, which he there sets down, but which he also observes to be very incorrect. I have not therefore inserted any of them in this collection.

Mr. *Smethwicke*, one of the earliest members of the *Royal Society*, communicated to the same in July 1670, the weights of a cubic inch of several different substances;

substances; said to have been formerly taken by Mr. *Reynolds* in the *Tower of London*. This gentleman was the same who composed several tables relating to the price of Gold and Silver, which were published in a book entitled *The Secrets of the Goldsmith's Art*, at *London* 1676, in octavo. These weights are expressed in decimals of an *Averdupois* Pound, are carried to 8 places of figures, and seem to have been carefully and accurately collected. I have therefore in the following tables reduced them to the common form, in order to give them their proper authority with the rest. I am ignorant whether these weights were ever before printed or not, neither can I give any account, after what particular manner the experiments were made, from which they were taken. They were communicated to me from the register-books of the *Royal Society*; and I shall only observe, that the absolute weight here assigned of a cubic inch of common water does not differ more than a small fraction of a grain, from the weight of the same afterwards determined by Mr. *Ward* of *Chester*.

The *Philosophical Society*, meeting at *Oxford*, directed several experiments to be made hydrostatically by their members, concerning the specific gravities of various bodies; which being digested into a table, were by Dr. *Musgrave* communicated to the *Royal Society* the 21st day of *March* 1684. soon after which they were printed in the 169th number of the *Philosophical Transactions*. These experiments were, according to Dr. *Musgrave*, made by Mr. *Caswell* and Mr. *Walker*: they are all originals,

and esteemed some of the most accurate that are extant.

The honourable *Robert Boyle*, at the end of his *Medicina hydrostatica*, first published at *London* in 1690, 8°. subjoined a table of the specific gravities of several bodies, accurately taken from his own hydrostatical experiments. Besides which, there are also in the same tract, and in other parts of his works, several experiments of this excellent author's, which he has given occasionally, together with the uses resulting from them. To such of these in the following collection, as were taken from the table just mentioned, I have barely annexed his name, but to such of the others as occurred, I have also added the volume, page, and column, of the late *folio* edition of his works in 1744, where the same are to be found. It may be noted, that in the first edition of the *Medicina hydrostatica*, there were several errors of the press. Such of them as I could discover by calculation, I have corrected in the following pages.

There is a table published under the name of *J. C.* in the 199th number of the *Philosophical Transactions*, A°. 1693: and this is evidently a supplement to that above-mentioned of the *Philosophical Society* meeting at *Oxford*. The experiments were, according to the initials *J. C.* made by the same curious person Mr. *John Caswell*, and are therefore of the same estimation as the others.

Mr. Homberg, of the *Royal Academy of Sciences* at *Paris*, read a memoir in 1699, wherein he took no
tice

tice of the expansion of all substances by heat, and the contraction of the same by cold: from whence it must follow, that the specific gravities of the same bodies would constantly be found less in the summer and greater in the winter. And this he shew'd from the experiments he had made upon several fluids, both in the summer and the winter-seasons, by means of an instrument he had contrived and called an *Areometer*, being a large phial, to which he had adjusted a long and slender stem, whereby he could to good exactness determine, when it was filled with equal bulks or quantities of the several fluids he proposed to examine. The result of his trials with this instrument he digested into a short table, which was printed in the memoirs of the *Academy* for the same year 1699. This table *John Caspar Eifenschmid* afterwards republished with several additions, in his tract *De Ponderibus et Mensuris*, printed at *Strasburg* in 1708, 8°. changing it to a more convenient form for his purpose, by reducing the different fluids therein named to the known bulk of a cubical *Paris* inch. So much of this table as I thought might be of service, I have here subjoined to the others in the following collection, but I have also made an alteration in the form, the better to fit it for general use, by omitting the absolute weights of the several bodies in summer and winter, and placing instead of them, after the name of each body a decimal number, expressing the proportion of its weight in winter to its weight in summer, supposed to be every-where represented by unity.

Sir

Sir *Isaac Newton* Knt. in his *Opticks* printed in 4°. at *London* 1704, gave a table of the specific gravities of several *diaphanous* bodies. The experiments were made by him with a view chiefly to optical enquiries, and to enable him to compare their densities with their several refractive powers: we may therefore be well assured that they were made by the great author with the most scrupulous care and exactness. The table consists of 22 articles.

John Harris D.D. in his *Lexicon Technicum*, first printed at *London* in 1704, fol. republished at large the several tables of specific gravities of the *Oxford Society* and *I. C.* from the *Philosophical Transactions*, and that of the honourable *Robert Boyle* from his *Medicina hydrostatica*, to which last he also added some experiments of his own, made as it seems with good accuracy. These are here extracted, and placed under his name in the following tables.

Mr. *John Ward* of *Chester*, in his *Young Mathematician's Guide*, first printed, as I take it in 1706, acquaints us, that he had himself for his own satisfaction, made several experiments upon the different specific gravities of various bodies; and that he was of opinion, that he had obtained the proportion of the weight that one body bears to another of the same bulk and magnitude, as nicely as the nature of such matter, as might be contracted or brought into a lesser body (*viz.* either by drying, hammering, or otherwise) would admit of. And he has accordingly

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given

given us in the said book the weight of a cubic inch of 24 different substances, both in *Troy* and *Averdupois* ounces and decimal parts of an ounce; which he further assures us requir'd more charge, care, and trouble, to find out nicely, than he was at first aware of. This table appears to have been well-esteem'd, and to have had the sanction of Mr. *Cotes's* approbation, by his taking it, when reduced to the common form, into that collection which he drew up for his own hydrostatical lectures.

Roger Cotes M. A. and *Plumian Professor of Astronomy and experimental Philosophy* at Cambridge, first giving about the year 1707 a *Course of Hydrostatical and Pneumatical Experiments*, in conjunction with Mr. *Whiston* in that University, drew up, for the use of that course, a very accurate Table of Specific Gravities, collecting from several places such experiments as he took to be most exact, and the best to be depended upon. And as the judgment of so great a man cannot but give a general reputation to such experiments as he had so selected, I have thought proper, in the following tables, to distinguish all such by the addition of the letter *C*, after the names of such persons from whom they first appear to have been taken, adding also the name of *Cotes* at length, to such others as I have not met with elsewhere, and which I therefore take to have been transcribed from the *memoranda* of his own experiments. This table of Mr. *Cotes's* used first to be given in *M.S.* to those who attended his lectures; but it was afterwards printed in a single sheet, relating to a *Course of Experiments* at Cambridge

in 1720, and since in Mr. *Cotes's Hydrostatical and Pneumatical Lectures*, when they were published at large in 8°. by his successor Dr. *Smith*, now the worthy Master of *Trinity College*. In these printed Lectures were inserted the gravities of Human Blood, its *Serum*, &c. from Dr. *Jurin*, instead of those that had before been made use of from Mr. *Boyle*.

Mr. *Francis Hauksbee*, now Clerk to the *Royal Society*, did, about the year 1710, begin, in conjunction with Mr. *Whiston*, who had then newly left the University, to give hydrostatical lectures &c. in *London*; for the purpose of which he reprinted in a thin volume in 4°, in which are the schemes of his experiments, Mr. *Cotes's* table of Specific Gravities above-mentioned. To which he added, from tryals of his own, the weights of Steel, soft, hard, and temper'd, which are printed with his name in the following Tables, as are also some other experiments, which he has since occasionally made, and communicated to me. Mr. *Cotes's* table, with the above-mention'd additions of Mr. *Hauksbee*, was afterwards again published by Dr. *Shaw*, in his *Abridgment of Mr. Boyle's Philosophical Works*, at *London*, 1725, 4°. vol. ii. p. 345.

John Freind M. D. at the end of his *Praelectiones Chymicae*, printed at *London* in 1709, 8°. has published some new tables of the Specific Gravities both of solid and fluid bodies, entirely taken from his own original experiments. And as these tables contain an account of a very useful set of bodies, upon which few or no other experiments have been made: it is great pity

pity that this truly learned and elegant writer was not more accurate in his tryals than he appears to have been. Many of his experiments having indeed been made in so lax and improper a manner, and so many errors having been committed in them, that one can not with security depend upon these tables, tho' containing otherwise facts one would so much desire to be truly informed about. I have however here inserted the several particulars of his two last tables, which immediately concern Specific Gravities, after correcting such errors in calculation as I could certainly come at : And I hope that I shall be excused for this free censure upon part of the works of a gentleman, who has so well deserved of the learned world, and acquired so just a reputation in it.

James Jurin, M. D. and several years Secretary of the *Royal Society*, gave, in N^o. 361 of the *Philosophical Transactions*, A^o. 1719, some original and very accurate experiments made by himself, upon the Specific Gravity of Human Blood, at several times during the six preceding years. These were accompanied with a very curious discourse, which has since been translated by himself, into *Latin*, and reprinted in his *Dissertationes Physico Mathematicæ*, Lond. 1732. 8^o.

This gentleman has also, in N^o. 369 of the same *Transactions*, obliged us with some very judicious and useful remarks, relating to the *caution to be used in examining the specific gravity of solids, by weighing them in water*; for want of attending to which, several sorts of bodies, such as human Cal-

culi, the substance of all woods, &c. have appeared, from their pores and small cavities filled up with air, to be considerably lighter than they really are.

John Woodward M. D. and Professor of Physic in *Gresham College*, had, as he acquaints us in several places of his works, made a great number of experiments upon the specific weights, of mineral and other fossil bodies, but which being probably contained in those of his papers which he ordered to be suppressed at his death, are thereby lost to the world, to which they would without all doubt have been very acceptable. All I have been able to pick up are a very few mentioned in the *Catalogue of the English Fossils in his Collection*, published since his decease, in 8°. at *London* 1729.

Mr. *Gabriel Fahrenheit* F. R. S. communicated, in N°. 383. of the *Philosophical Transactions*, *A Table of the Specific Gravities of 28 several substances*, from hydrostatical experiments of his own, made with great care and exactness; to which he subjoined some observations upon the manner in which his trials were performed, together with a description of the instruments in particular which he made use of to examine the gravities of Fluids. To some of his experiments which he thought required a greater nicety, he has affixed an asterisk in his table, signifying such to have been adjusted to the temperature of the air, when his Thermometers stood at the height of 48 degrees. This gentleman, who is well known by the reputation of his Mercurial Thermometers, which he made with
great

great curiosity, and which are now generally used, was in *England* in the year 1724.

Professor *Peter van Musschenbroek*, of *Utrecht*, published in his *Elementa Physicæ* at *Leyden* in 8°. 1734. a large table of Specific Gravities, which he afterwards yet somewhat further enlarged in his *Essai de Physique* in *French*, at *Leyden* 1739. 4°. This table contains almost all the preceding ones, but without the names of the authors from whom they were collected. I have among those which follow inserted, under this author's name, such experiments as I had not before met with elsewhere: making use of the *Latin* edition as the more correct, except in such articles which are only to be found in the *French*.

Mr. *John Ellicott* F. R. S. having an opportunity in the year 1745. to examine the weight of some large Diamonds, he accordingly, with the utmost care, and with exquisite assay-scales which very sensibly turned with the 200th part of a grain, took the specific gravities of 14 of those Diamonds, 4 of which came from the *Brazils*, and the other 10 from the *East Indies*. These experiments he communicated to the President of the Royal Society, who caused them to be read at one of their meetings, and afterwards published them in N°. 476. of the *Philosophical Transactions*. Among these *Brazilian* Diamonds, one was of the absolute weight of 92,425, another of 88,21; and among the *East-Indian* ones, one of 29,525 *Troy* grains. And as the size of these stones made them much

fitter for these enquiries, than any others which had probably ever before been used for the same purpose, so the known accuracy of the author, the goodness of his instruments, and the consistency of all his experiments, sufficiently shew the specific gravities he has delivered in his paper, may entirely be depended upon.

The same curious person also communicated the Specific Gravities of fine and standard Gold, published under his name in the following tables, and which were deduced from experiments he was so kind as to make on purpose at my request.

As I have just had occasion to mention Diamonds, it may possibly not be foreign to the purpose here to take some notice of the Diamond Carat weight, used among jewellers, which weight was originally the Carat or 144th part of the *Venetian* ounce, equal to 3,2 *Troy* Grains, but which is now, for want of an acknowledged standard, somewhat degenerated from its first weight. I have myself found it, upon a medium of several experiments, equal to 3,17 *Troy* Grains; and I have the rather taken notice of this weight here, because there happens to be a mistake about it, both in Dr. *Arbuthnot's* and Mr. *Dodson's* tables, who have set down as it seems the number of Diamond Carats in a *Troy* Ounce, instead of the weight of the Diamond Carat itself. This Carat is again divided into four of its own Grains, and those into halves and quarters, commonly called the eighths and sixteenths of a Carat: and thus the largest of the Diamonds just above-mentioned, weighed, in the jewellers phrase, better than 29 Carats and almost half a Grain.

Mr *James Dodson*, in his book called *The Calculator*, printed in 8°. at *London* in 1747, has inserted a useful table of Specific Gravities, in which he has by the first initial letter of their names distinguished the several authors he has quoted: and amongst these are several new experiments marked with an *L*, which I am told were communicated from his own trials, by Mr. *Charles Labelye*, engineer, and which concern particularly the weights of several sorts of stone and other materials used in building. These I have also distinguished by an *L*. as they stand in Mr. *Dodson's* book.

Mr. *Geo. Graham*, F.R.S. made for me, at the request of a friend, some accurate trials upon the weight, of Gold and Silver, both when reported fine, and when reduced to the *English* Standard: all which I have inserted under his name in the following tables. Wherein I have besides reported some other single Experiments which I occasionally met with, from *Frederick Stare* M.D. *John Keill* of *Oxford*, M.D. *Stephen Hales* D.D. and *Edward Bayley* of *Havant* in *Hampshire*, M.D.

Rickard Davies M.D. I have lastly to this Collection of Experiments added some of my own, which I endeavoured to make with as much accuracy, as the instruments I was provided with would allow of. My hydrostatical Balance was one constructed several years since by Mr. *Francis Hauksbee*, which I have constantly found to turn sensibly with half a grain: and the bodies upon which I made most of my trials, were taken from a collection of the *Materia Medica* formerly made by
Signor

Signor *Vigani*, and still preserved in the library of *Queen's College* in *Cambridge*.

TABLE I.

Of Metals.

© GOLD, fine. <i>Ward, C.</i>	19.640
A Medal esteemed to be near fine Gold	
<i>Ƴ. C.</i>	19.636
Or d'essai, ou de Coupelle. <i>Muschenbr.</i>	19.238
Fine Gold hammer'd. <i>Ellicot.</i>	19.207
D°. an ingot i ^o accounted, and again refined with Antimony. <i>Ellicot.</i>	19.184
D°. the ingot itself just mention'd. <i>Ellicot.</i>	19.161
A Medal of the Royal Society, reported fine Gold. <i>Graham.</i>	19.158
A gold medal of Qu. Eliz. <i>Ƴ. C.</i>	19.125
D°. of Qu. Mary. <i>Ƴ. C.</i>	19.100
Aurum. <i>Fahrenheit.</i>	19.081
Id. <i>Ghetaldus.</i> Aurum purum. <i>Bacon</i> (ex hyp.)	19.000
A gold Coin of Alexander's. <i>Ƴ. C.</i>	18.893
Gold. <i>Reynolds.</i>	18.806
Aurum. <i>Villalpandus. Petitus.</i>	18.750
Standard Gold (by which is understood Gold of 22 Carats, or such of which our Guineas are intended to be coined).	
<i>Ƴ. C. Ward. C.</i>	18.888
An old Jacobus. I suppose the scepter'd broad piece. <i>Harris.</i>	18.375
A Mentz gold Ducat. <i>Ƴ. C.</i>	18.261
Aureus	

Aureus Ludovicus. <i>Muschenbr.</i>	18.166
A five Guinea piece of K. James II. 1687. with an Elephant. <i>Graham.</i>	17 933
A Portugal piece of 3l. 12s. 1731. sup- posed to be nearly the same as Stand- ard. <i>Graham.</i>	17.854
Guineas, ten weighed together. <i>Davies.</i>	17.800
D ^r . on a mean of 7 trials upon those of different reigns. <i>Ellicot.</i>	17.726
A piece of Gold Coin of the Common- wealth. <i>Harris.</i>	17 625
Guineas two new ones. <i>Hauksbee.</i>	17.414
A Grain of Scotch Gold, such as Nature had made it. <i>Boyle V. 30. b. . . 12$\frac{2}{7}$</i>	12.286
Electrum, a British Coin. <i>J. C.</i>	12.071

QUICKSILVER. <i>Mercurius crudus.</i>	
<i>Freind.</i>	14.117
Mercury Spanish. <i>Boyle V. 10. b.</i>	
Mercure sublimé 511 fois. <i>Muschenb.</i>	14.110
Quicksilver. <i>Oxford Soc.</i>	14.019
D ^r . <i>Ward. C.</i> revived from the Ore. <i>Boyle.</i>	14.000
Fine Mercury. <i>L.</i>	13 943
Quicksilver, another Parcel. <i>Oxf. Soc.</i>	13.593
Mercure amalgamé avec de l'Argent, affiné et sublimé 100 fois. <i>Muschenb.</i>	13.580
Mercurius. <i>Fahrenheit.</i>	13.575*
Argentum vivum. <i>Ghetaldus. 13$\frac{4}{7}$</i>	13.571
Mercure amalgamé avec de l'Or affiné, et sublimé 100 fois; le même mêlé avec du Plomb, ensuite converti en poudre et revivifié. <i>Musch.</i>	13.550 Coarse

Coarse Mercury. <i>L.</i>	13.512
Mercurius. <i>Petitus.</i>	13.406
Quicksilver. <i>Reynolds.</i>	13.147

LEAD. <i>Reynolds.</i>	11.856
Plumbum. <i>Villalpand.</i>	11.650
Id. <i>Ghetaldus</i> $11\frac{1}{2}$.	11.500
Id. <i>Bacon.</i>	11.459
Lead. <i>Harris.</i>	11.420
Hardest Lead. <i>L.</i>	11.356
Plumbum. <i>Fahrenheit.</i>	11.350
Lead. <i>Oxford Soc. Ward.</i>	11.345
Plumbum. <i>Petitus.</i>	11.343
Lead. <i>Harris.</i> (an ordinary Piece)	11.330
D°. <i>Cotes.</i>	11.325
Plumbum Germanicum. <i>Muschenb</i>	11.310
Cast Lead. <i>L.</i>	11.260

SILVER, fine. <i>Ward. C.</i>	11.091
A Medal of the Royal Society, reported fine Silver. <i>Graham.</i>	10.484
Argentum. <i>Fahrenheit.</i>	10.481
Silver. <i>Reynolds.</i>	10.432
Argentum. <i>Villalpandus.</i>	10.400
Id. <i>Ghetaldus.</i> $10\frac{1}{2}$.	10.333
Id. <i>Bacon.</i>	10.331
Id. <i>Petitus.</i>	10.219
Sterling or Standard Silver (that is, Silver 11 oz. 2dwt. in the pound fine) A half crown of K. William's Coin. <i>Harris.</i>	10.750
D°. struck into money. <i>L.</i>	10.629
D°. <i>J. C. Ward. C.</i>	10.535
D°. Cast. <i>L.</i>	10.520

A new Crown-piece. 1746. LIMA
under the head. *Graham.*

10.284

2 COPPER. <i>Reynolds.</i>	?	?	9.127
Cuprum. <i>Villalpandus.</i>	.	.	9.100
Æs. <i>Ghetaldus.</i> Rose Copper. <i>Ward.</i>			
C. Fine Copper. <i>L.</i> An old Cop- per Halfpeny, Charles II's Coin.			
<i>Harris.</i>	.	.	9.000
Copper in Half-pence. <i>L.</i>	.	.	8.915
Æs; Cuivre. <i>Petitus.</i>	.	.	8.875
Cuprum. <i>Bacon.</i>	.	.	8.866
Copper. <i>Oxf. Soc.</i>	.	.	8.843
Cuprum Suecicum. <i>Fahrenheit.</i>	.	.	8.834
Id. Japonense. <i>Fahrenheit.</i>	.	.	8.799
Id. Suecicum. <i>Muschenbr.</i>	.	.	8.784
Common Copper. <i>L.</i>	.	.	8.478

BRASS. An old brass gold weight marked

xxxiii. <i>Harris.</i>	.	.	8.830
Aurichalcum. <i>Bacon.</i>	.	.	8.747
A Piece of hammer'd Brass. <i>Harris.</i>	.	.	8.660
Æs, Airin, Calaminæ mixtum. <i>Petitus.</i>	.	.	8.437
Aurichalcum. <i>Fahrenheit.</i>	.	.	8.412
Brass hammer'd. <i>J. C.</i> Plate Brass.			
<i>Ward.</i>	.	.	8.349
Wrought Brass. <i>J. C.</i>	.	.	8.280
Cast Brass. <i>L.</i>	.	.	8.208
D°. <i>J. C.</i> <i>Ward.</i>	.	.	8.100
D°. <i>Cotes.</i>	.	.	8.000
Brass hammer'd. <i>Reynolds.</i>	.	.	7.950
D°. Cast. <i>Reynolds.</i>	.	.	7.905
A Piece of cast Brass. <i>Harris.</i>	.	.	7.666

M m m

3 IRON.

♂ IRON. Ferrum. <i>Villalpandus</i> .	8.086
Id. <i>Ghetaldus</i> .	8.000
Iron, forged. <i>Reynolds</i> .	7.906
Ferrum. <i>Petitus</i> .	7.875
Id. <i>Bacon</i> .	7.837
Spanish bar Iron. <i>L</i> .	7.827
Swedish D°. <i>L</i> .	7.818
Ferrum. <i>Fahrenheit</i> .	7.817
Iron. <i>Cotes</i> .	7.645
D° of a key. <i>J.C. Common Iron. Ward</i> .	7.643
A piece of hammer'd Iron, perhaps part Steel. <i>Harris</i> .	7.600
Iron cast. <i>Reynolds</i> .	7.520
D° cast. <i>L</i> .	7.135
Softest cast Iron or Dutch Plates. <i>L</i> .	6.960
STEEL. <i>J. C. Ward</i> .	7.852
D°. <i>Cotes</i> .	7.850
D°. Spring Temper. <i>Hawksbee</i> .	7.809
D°. Nealed soft. <i>L</i> .	7.792
D°. Soft. <i>Hawksbee</i> .	7.738
D°. Hard. <i>Hawksbee</i> .	7.704
D°. Harden'd. <i>L</i> .	7.696
♂ TIN. <i>Reynolds</i> .	7.617
Stannum. <i>Bacon</i> .	7.520
Id. <i>Villalpandus. Freind</i> .	7.500
Etain d'Angleterre. <i>Musschenbr.</i>	7.471
Stannum. <i>Ghetaldus</i> . $7\frac{2}{3}$	7.400
Id. Provinciae Indiae Or. Malacca. <i>Fahren.</i>	7.364
Block Tin. <i>Oxf. Soc. Ward. C.</i>	7.321
Stannum Anglicanum. <i>Fahrenheit</i> .	7.313
Id.	

Id. commune. <i>Petitus.</i>	.	.	.	7.312
Id. purum. <i>Petitus.</i>	.	.	.	7.170
Block or Grain Tin. <i>L.</i>	.	.	.	7.156

Notes and Observations.

As I thought the uses that might be made of these Tables, either in business or in philosophy, would best be illustrated by a few short notes, I have therefore here occasionally inserted such observations as occurred to me, whilst I was revising them for the press: and as many of these related chiefly to the present defects of my tables, those I thought would probably be of service, to such as might hereafter take the trouble of improving or correcting them.

As the particulars contained in the Tables were extracted from different books, at different times, and at first only intended for my own private use, I was not solicitous to preserve one uniform language, but generally set down every experiment in my common-place, in the words of the author I took it from: and as I have since found, that by a translation I might sometimes happen not so justly to represent the body intended, I have upon the whole judged it best, here also to transcribe them in the same languages in which they were at first delivered.

To make experiments of this sort with a sufficient degree of accuracy requires a pretty deal of care and pains: and as in such as I have made myself, I have found great conveniency in the use of decimal weights, preferably to those of the common form,

I would also recommend the use of such to others, who shall please to employ themselves in the like enquiries. - Those I have provided for myself have a *Troy* Ounce for their *integer*, and my least weight is the thousandth part of that quantity, differing consequently from the half of a *Troy* Grain only as 24 does from 25, which is inconsiderable so far as those small weights are concerned. My four smallest are respectively of 1, 2, 3 and 4 of those thousandth parts, and together make 10, or an unit of the next denomination, that of the 100th part of an ounce. I then have four others, making 1, 2, 3 and 4 100^{ths}, and together the unit of of the next denomination, or one tenth of an ounce, and so on. By these I save the trouble of reducing the common weights to their lowest denomination in every experiment, and sometimes perhaps avoid making mistakes in that very trifling work.

Whenever two or more original writers nearly concur in their experiments upon any subject, the Gravity so deduced may be well depended upon. But where they differ remarkably it must either be imputed to the unequal gravity of the subject itself, or to some error in the tryals, which may easily happen in matters that depend on the observation of so many minute particulars. All those cases that so sensibly differ would well deserve to be re-examined.

The first Table above, that of Metals, as it is composed of the most perfect and uniform bodies in nature, seems capable of being adjusted with the greatest precision, both with relation to the pure
Metals

Metals themselves, and to the several degrees of their mixtures one with another, if experiments in all these cases were but made with a sufficient degree of accuracy.

Gold, in the experiments I have made myself, I could never find to come up to the weight assigned it in some of the former tables, and particularly those I have made upon our own coin, and some others have always remarkably fallen short of the weight assigned to the Standard in those same tables. I have inserted that trial in which I found Guineas to come out best; and I may venture to affirm, that that experiment, in particular, was made with as much accuracy as my instrument was capable of, the Pieces were all washed in soap and water, cleaned with a brush, and the air-bubbles well freed and the like. That experiment is besides abundantly confirmed since, by the exact trials lately made by Mr. *Graham* and Mr. *Ellicot*, which were performed with the greatest care; and the fine Gold also mentioned by the last was chosen and prepared with the greatest curiosity.

It may be observed, that the gold medals of *Q. Eliz.* and *Q. Mary*, quoted by *J. C.* were, without doubt, the large Sovereigns of those Queens, which were of the old Standard of *England*, or of gold appointed to be 23 carats, 3 grains and a half fine: That the *Mentz* Ducat, mentioned by the same, if it was one of those *ad Legem Imperii*, which are always in their own mints affirmed to be fine, come out considerably too light: and that the gold coin of the Commonwealth, and the pistoles of *France*, were like our present gold money of the goodness of 22 carats.

Mercury

Mercury is placed in this table among the Metals, by reason of its near agreement with those bodies in its specific gravity; tho' it otherwise so widely differs from them in most of its properties.

Brass is considerably condensed by hammering; whether Gold, Silver, and the other Metals are also condensed in like manner, hardly appears yet to have been sufficiently tried.

Of the mixed Metals, hardly any except Brass, appear to have had their specific gravities very carefully ascertained: bell-metal, princes metal, however, and some others, might deserve to be examined in that particular.

It might possibly be queried also, whether several mixed Metals do not either rarify or condense upon mixture, so as thereby to acquire a different specific gravity, than the natural law of their composition, at first seems to require.

It may lastly be observed, that the specific gravities of all the known Metals are such, as that none of them come up to 20 times the weight of common water, or fall sensibly below 7 times the same weight.

TABLE II.

*Of Minerals, Semimetals, Ores, Preparations
and Recrements of Metals, &c.*

BISMUTH.	J.C.	9.859
D°. Cotes	9.700
D°. or Tinglafs.	Boyle.	9.550
						Tynglafs,

Tungst. <i>Reynolds</i>	7.951
Marcasita alba. <i>Fahrenheit.</i>	9.850
Mineral, Cornish, shining like a Marcasite.	
<i>Boyle.</i>	9.06
Calx of Lead. <i>Boyle.</i>	8.940
Spelter Solder. <i>J. C.</i>	8.362
Spelter. <i>J. C.</i>	7.065
Cinnabar common. <i>Boyle.</i>	8.020
Cinnabaris factitia. <i>Muschenb.</i> (if not a mistake for the last experiment)	8.200
Cinnabar native, breaking in polish'd sur- faces like Talc. <i>Davies.</i>	7.710
D°. Persian, breaking rough. <i>Davies.</i>	7.600
D°. native. <i>Boyle.</i>	7.576
Cinnabaris nativa. <i>Muschenb.</i>	7.300
Cinnabar native, very sparkling. <i>Boyle.</i>	7.060
D°. native from Guinea. <i>Davies.</i>	6.280
Cinnabar of Antimony. <i>Harris.</i>	7.060
D°. another piece. <i>Harris.</i>	7.043
D°. <i>Boyle.</i>	7.030
Cinnabar Antimonii. <i>Freind.</i>	6.666
Cinnabre d'Antimoine. <i>Muschenb.</i>	6.044
Lead Ore, rich, from Cumberland. <i>Boyle.</i>	7.540
D°. <i>Boyle.</i>	7.140
The reputed Silver Ore of <i>Wales.</i> <i>J. C.</i>	7.464
The Metal thence extracted. <i>J. C.</i>	11.087.
Regulus Antimonii. Item Martis et Veneris.	
<i>Freind.</i>	7.500
Id. <i>Fahrenheit.</i>	6.622
Id. <i>Harris.</i>	6.600
Id. per se. <i>Davies.</i>	4.500
Silver Ore, choice. <i>Boyle.</i>	7.000
D°.	

D°. another piece from Saxony.	<i>Boyle.</i>	4.970
Lithargyrus Argenti.	<i>Freind.</i>	6.666
Lithargyrium Argenti.	<i>Muschenb.</i>	6.044
Id. Auri.	<i>Freind.</i>	6.316
Id. Auri.	<i>Muschenb.</i>	6.000
Minera Antimonii.	<i>Davies.</i>	5.810
Cuprum calcinatum.	<i>Freind.</i>	5.454
Glass of Antimony.	<i>Newton. C.</i>	5.280
Vitrum Antimonii.	<i>Freind.</i>	5.000
Id. per se.	<i>Boyle.</i>	4.760
Tin Ore, choice.	<i>Boyle.</i>	5.000
D°. black, rich.	<i>Boyle.</i>	4.180
New English Tin Ore, Mr. Hubert's.	<i>Boyle.</i>	4.080
Tutty, a piece.	<i>Boyle.</i>	5.000
Tutia.	<i>Muschenb.</i>	4.615
Lapis Calaminaris.	<i>Freind.</i>	Lapis cæruleus
Namurcensis.	<i>Muschenb.</i>	5.000
Id.	<i>Boyle.</i>	4.920
Loadstone.	<i>Boyle V. 6. b.</i>	4.930
Magnes.	<i>Petitus.</i>	4.875
A good Loadstone.	<i>Harris.</i>	4.750
Marcasites, one more shining than ordinary.	<i>Boyle.</i>	4.780
A Golden Marcasite.	<i>J. C.</i>	4.589
Marcasites, from Stalbridge.	<i>Boyle.</i>	4.500
D°. <i>Boyle.</i>		4.450
Antimonium Hungaricum.	<i>Muschenbr.</i>	4.700
Antimony, good, and supposed to be Hun-		
garian.	<i>Boyle.</i>	4.070
D°. crude, which seemed to be very good.	<i>Harris.</i>	4.058
		Antimonium

Antimonium crudum. <i>Freind.</i>	4.000
Id. <i>Davies.</i>	3.960
Black Sand, commonly used on writing.	
<i>Boyle. V. 33. b.</i>	4.600
Crocus Metallorum. <i>Muschenb.</i>	4.500
Id. <i>Freind.</i>	4.444
Hæmatites. <i>Muschenbr.</i>	4.360
Id. <i>Boyle. V. 6. a.</i>	4.150
D°. English. <i>Boyle.</i>	3.760
Copper Ore, rich. <i>Boyle.</i>	4.170
D°. <i>Boyle.</i>	4.150
Copper-stone. <i>Boyle.</i>	4.090
Emeri. <i>Boyle. V. 26. b.</i>	4.000
Manganese. <i>Boyle.</i>	3.530
A blew Slate with shining particles. <i>J. C.</i>	3.500
Iron Ore, a piece burnt or roasted. <i>Harris.</i>	3.333
Cerussa. <i>Item Chalybs cum Sulphure. pp.</i>	
<i>Freind.</i>	3.158
Lapis Lazuli. <i>J. C.</i>	3.054
D°. <i>Boyle. V. 6. b.</i>	3.000
D°. <i>Boyle.</i>	2.980
Gold Ore. <i>Boyle. V. 29. b.</i>	2.910
D°. not rich, brought from the East Indies.	
<i>Boyle.</i>	2.652
Another Lump of the same. <i>Boyle.</i>	2.634
A Mineral Stone, yielding 1 part in 160	
Metal. <i>J. C.</i>	2.650
The Metal thence extracted. <i>J. C.</i>	8.500.
Pyrites homogenea. <i>Fahrenheit.</i>	2.584
Black Lead. <i>Boyle. V. 27. a.</i>	1.860
Æs viride. <i>Freind.</i>	1.714
Plumbumustum. <i>Freind.</i>	1.666
N n n	The

The second Table is composed of subjects no way strictly allied to each other, either by their gravities, or their other essential properties; and perhaps they might better, on that account, have been divided into different tables.

The bodies themselves are chiefly of an uncertain and heterogeneous nature; being so far as appears composed of different elements, and those also combined in various proportions, such as Sulphur and Arsenic, joined with Stone, Metal, and the like: and from these several degrees of mixture it must follow, that most of these kinds of bodies, tho so far similar as to be called, by the same names, yet must necessarily admit of a considerable latitude in their specific gravities. Many useful deductions may nevertheless be drawn from those considerations, relating to the comparative goodness &c. of such bodies.

Cinnabar native appears to be a compound of Mercury and Sulphur, with a portion of earthy or stony matter; and that which is heaviest must abound most with the Mercury. The different appearances which this body makes, would also give us a suspicion that there are other varieties in its composition, besides those just taken notice of: some sorts of Cinnabar, such as the *Hungarian*, breaking into polished planes and squares like Talc, whilst others, like the *Persian* of this table, break rough and with shining granula or mica; and that without any considerable difference in their gravities.

By the factitious Cinnabar it may be determined, what proportion of Mercury will so incorporate with Sulphur, as to make up an uniform body.

Antimony

Antimony may in like manner be considered as a composition of its Regulus and Sulphur.

The black sand used on writing is said by Mr. *Boyle* to be a rich Iron Ore: he also says that *Emeri*, Loadstone, and all such ponderous stones, contain some kind of metal, which he had himself separated from them. IV. 120. *a*.

The great variety of Ores of all kinds well deserve to be accurately examined, for the sake of the many conclusions that may be drawn from thence, concerning the natures of concrete bodies, and for many other purposes in Metallurgy. But I have as yet met with a very small number of experiments upon these substances. Dr. *Woodward* has indeed mentioned a great many observations of this sort which he had made, and kept exact registers of: but as they were probably among those papers which he order'd to be destroy'd at his death, we must look upon them as now lost to the world.

The Marcasites and Pyrites are very uncertain and strange kinds of bodies, their gravities are often very great: a Marcasite here taken from *Fahrenheit* was found nearly to equal the heaviest mineral Bismuth itself; and yet it is very seldom that any Metal or semimetal can be obtained in any quantity from these substances, all that is in them being usually destroyed, and carried away by their sulphur.

Black Lead is also a very odd kind of Mineral, having all the appearance of a Semimetal, and yet falling short even of the weight of common earth.

The Semimetals generally exceed in their specific gravity even the baser Metals themselves.

It may be observed, that it appears by this table, that the specific gravities of ores, including the metallic stones, are usually found to lie between 7 and 3 times the weight of water. Lead and Silver ores are the heaviest, those of Copper, Tin and Iron being considerably lighter. The Gold Ore we have an account of must be so poor as hardly to be worth taking any notice of: but we have in general too few of these experiments, to draw any certain conclusions from them.

TABLE III.

Of Gems, Chrystals, Glass, and transparent Stones.

GRANATE, Bohemian. <i>Boyle.</i>	4.360
Granate. <i>J. C.</i>	3.978
Granati minera. <i>Boyle.</i>	3.100
A Pseudo-Topazius, being a natural pellucid, brittle, hairy stone, of a yellow colour.	
<i>Newton. C.</i>	4.270
Sapphires. <i>Davies.</i>	4.090
A Sapphire very perfect, but rather pale.	
<i>Hauksbee.</i>	4.068
Glass, blue in sticks from Mr. Seale.	
<i>Hauksbee.</i>	3.885
D°. whitest, from Mr. Seale. <i>Hauksbee.</i>	3.380
D°. clear chrystal. <i>Cotes.</i>	3.150
D°. blue plate, old. <i>Hauksbee.</i>	3.102
D°. plate. <i>L.</i>	2.942
	D°.

Do, old looking-glass plate of a light colour. <i>Hauksbee.</i>	2.888
Do. green. <i>Freind.</i>	2.857
Do. green bottle. <i>Hauksbee.</i>	2.746
Do. of a bottle. <i>Oxf. Soc.</i> It. a blue paste. <i>Hauksbee.</i>	2.666
Do. common green. <i>Hauksbee.</i>	2.620
Do. deep green old. <i>Hauksbee.</i>	2.587
Do. vulgar. <i>Newton. Ward.</i>	2.580
Vitrum Venetum. <i>Freind.</i>	1.791
An oriental Cat's-Eye, very perfect. <i>Hauksb.</i>	3.703
A Diamond, yellow, of a fine water, somewhat paler than the jonquille. <i>Hauksbee.</i>	3.666
Do. white of the second water. eau celeste. <i>Hauksbee.</i>	3.540
Do. East Indian, the heaviest of many. <i>Ellicot.</i>	3.525
Do. the lightest of many. <i>Ellicot.</i>	3.512
Do. Brasilian, the heaviest of many. <i>Ellicot.</i>	3.521
Do. the lightest of many. <i>Ellicot.</i>	3.501
Do. the mean of all his experiments. <i>Ellicot.</i>	3.517
Do. <i>Newton. C.</i>	3.400
Diamond Bort, of a bluish black, with some little adhering foulness. <i>Hauksbee.</i>	3.495
A Jacinth of a fine colour, but somewhat foul. <i>Hauksbee.</i>	3.637
A Chrysolite. <i>Hauksbee.</i>	3.360
Chrystal cubic, supposed to contain lead. <i>Woodward.</i>	3.100
Chrystal from Castleton in Derbyshire, having the double refraction. <i>Hauksbee.</i>	2.724
Chrystal of Island. <i>Newton. C.</i>	2.720
Chrystallum	

Chryſtallum diſdiaclaſticum.	J. C.	2.704
Chryſtallus de Rupe.	Fahrenheit.	2.669
Chryſtal rock.	J. C. Boyle III. 229. b.	2.659
Do. a large ſhoot.	Hauksbee.	2.658
Do. of the rock.	Newton. C. Ir.	
Chryſtal in the lead-mines near Works- worth.	Woodward.	2.650
Do. Hauksbee.		2.646
Do. pure pyramidal, ſuppoſed to contain Tin.	Woodward. 2.5 or	2.400
Chryſtallus.	Petitus.	2.287
Chryſtal.	Boyle.	2.210
Talc, Jamaican.	Boyle.	3.000
Do. Venetian.	Boyle.	2.730
Do. J. C.		2.657
Do. Engliſh.	Woodward.	2.600
Do. a piece like Lapis Amianthus.	Boyle.	2.280
A red paſte.	J. C.	2.842
A Braſile pebble, foul and feather'd.	Hauksb.	2.755
Do. a fragment uncut.	Hauksbee.	2.676
Do. cut.	Hauksbee.	2.591
Jasper, ſpurious.	J. C.	2.666
A Corniſh Diamond cut.	Hauksbees	2.658
A Water Topaz, very perfect, but ſaid not to be Oriental.	Hauksbee.	2.653
Pebble pellucid.	J. C.	2.641
Briſtol Stone.	Davies.	2.640
Hyacinth, ſpurious.	J. C.	2.631
Selenites.	J. C.	2.322
Do. Newton.		2.252

As the mean gravity of Chryſtal appears, by the foregoing table, to be little more to that of water than

than as two and a half to one; it may well be suspected, that the Granate, Pseudo-Topazius, Sapphire, and such other Gemms which greatly exceed Chrystal in weight, do contain a considerable portion of some sort of Metal in their composition: as was observed of these bodies by Dr. *Woodward*, in his Method of Fossils, p. 24.

As to the white Sapphire, which is reputed by Dr. *Woodward* to be a species of Gemm intermediate between Chrystals and the Diamond in hardness, I have not yet obtained any good account of its specific gravity.

The weight of the Diamond is ascertained in No. 476. of the *Philosophical Transactions*, where it appears, that by experiments made with the greatest care, by Mr. *John Ellicot* F.R.S. with most exact instruments, and upon 14 different Diamonds, some of them very large, brought from different places, and having the greatest varieties of colour and shape possible; they were all found to agree in weight to a surprising degree of accuracy, being all somewhat above three times and a half the weight of common water.

This indeed differs very sensibly from what had been found in some former experiments, but it is hardly probable that those had been made upon Diamonds of so large a size as these: Mr. *Boyle* who found their weight less than 3 times that of common water, has himself told us in the same place, V. 83. b. that the stone he made use of only weighed about 8 grains. And tho no doubt can be made of the exactness of Sir *Isaac Newton's* experiment,

riment, by which also the specific weight of the Diamond came out less than Mr. *Ellicot's*, yet it may well be question'd, whether Sir *Isaac* had, at the time when he made his trials, either so many or so perfect and weighty stones, as a favourable opportunity offered to this last gentleman. I shall therefore only observe, that, admitting this last to be the true specific weight of the Diamond, the refractive power of the same, in proportion to its density, should in Sir *Isaac Newton's* table be lessened from 14556 to 14071; which would still be greater than what is found in any other body; but is upon the whole more conformable to the general law of that table.

Sir *Isaac Newton* conjectured a Diamond to be an unctuous substance coagulated, and found it to have its refractive power nearly in the same proportion to its density as those of Camphire, Oyl-Olive, Lintseed Oyl, Spirit of Turpentine and Amber, which are fat sulphureous unctuous bodies: all which have their refractive powers two or three times greater in respect to their densities than the refractive powers of other substances in respect of theirs. Yet must it be allowed that a Diamond suffers no change by heat in any degree, contrary to the known property of Sulphurs; and as it is most reasonable in our Philosophy to treat such bodies as simple, in which we are not able to produce any change or separation of parts, we must therefore on that account consider a Diamond as a simple body and of the Chryselline kind.

Glass, which is a factitious concrete of Sand and Alkaline Salt, is nearly found to assume the mean gravity of Stones and Chrystals.

If,

If there is no mistake in the gravity of what Dr. *Freind* calls *Vitrum Venetum*, it differs very remarkably from all other kinds of Glass.

I do not know whether the Jasper and Hyacinth spurious of *J.C.* are to be understood as natural or artificial Gemms.

TABLE IV.
Of Stones and Earths.

Sardachates. <i>J. C.</i>	3.598
Lapis scissilis cæruleus. <i>Musschenbr.</i> (qu. if not the same experiment mentioned before pag. 447. <i>a blew slate with shining particles. J. C.</i>)	3.500
Cornelian. <i>Boyle.</i>	3.290
Do. <i>J. C.</i>	2.563
A Hone. <i>J. C.</i>	3.288
Do. to let razors on. <i>Harris.</i>	2.960
Marmor. <i>Petitus.</i> (probably some mistake in the experiment.)	3.937.
Marble. <i>Reynolds.</i>	3.026
Do. white. <i>Hauksbee.</i>	2.765
Do. white Italian, of a close texture vi- sibly.	2.718
Do. white. <i>Boyle.</i> fine. <i>Ward. C.</i>	2.710
Do. white Italian, tried twice. <i>Oxford Soc.</i>	2.707
Do. black Italian. <i>Oxford. Soc.</i> veined. <i>L.</i>	2.704
Do. black. <i>Hauksbee,</i>	2.683
Do. Parian. <i>L.</i>	2.560
Lapis Amianthus, from Wales. <i>J. C.</i>	2.913
Turquoise, one of the old rock, very perfect. <i>Hauksbee.</i>	2.908
Turquoise Stone. <i>J. C.</i>	2.508
O o o	Lapis

Lapis Nephriticus.	J. C.	2.894
Corallium rubrum.	Freind.	2.857
Corall.	J. C.	2.689
Do. red.	Boyle V. 7. a.	2.680
Do.	Boyle.	2.630
Do. white, a fine piece.	Boyle.	2.570
Do. white, another piece.	Boyle.	2.540
Emeril Stone, a solid piece.	Hauksbee.	2.766
Paving Stone.	Reynolds.	2.708
Do. a hard sort from about Blaiden.		
Oxf. Soc.		2.460
A Whetstone, not fine, such as cutlers use.		
Harris.		2.740
Pellets, vulgarly called Alleys, which boys play withal.	Hauksbee.	2.711
English Pebble.	L.	2.696
Lapis Judaicus.	Boyle.	2.690
Id.	Freind.	2.500
Maidstone Rubble.	L.	2.666
Marbles, vulgarly so called, which boys play withal.	Hauksbee.	2.658
Morr Stone.	L.	2.656
Agate.	Boyle.	2.640
Do. German, for the lock of a gun.		
Hauksbee.		2.628
Do. English.	J. C.	2.512
Lapis,	Petitus.	2.625
Flint, black, from the Thames.	Hauksbee.	2.623
Flint Stone.	L.	2.621
A round pebble-stone within a flint.		
Harris.		2.610
East Indian blackish. Item, an English one.		
Boyle. III. 243. a.		2.600
Do.		

Do. <i>Oxford Soc.</i>	2.542
Corallachates. <i>J. C.</i>	2.605
Purbeck Stone. <i>L.</i>	2.601
Freestone. <i>Reynolds.</i>	2.584
Portland Stone. <i>L.</i>	2.570
Do. white for carving. <i>L.</i>	2.312
Grammatias Lapis. <i>J. C.</i>	2.515
Onyx Stone. <i>J. C.</i>	2.510
Slate Irish. <i>Boyle.</i> Lapis Hibernicus.	
<i>Davies.</i>	2.490
Wood petrified in Lough Neagh. <i>J. C.</i>	2.341
Osteocolla. <i>Boyle.</i>	2.240
Heddington Stone. <i>L.</i>	2.204
Allom Stone. <i>Boyle.</i>	2.180
Bolus Armena. <i>Freind.</i>	2.137
Hatton Stone. <i>L.</i>	2.056
Burford Stone, an old dry piece. <i>Oxford Soc.</i>	2.049
Heddington Stone, that of the soft lax kind.	
<i>Oxford Soc.</i>	2.029
Terra Lemnia. <i>Freind.</i>	2.000
Brick. <i>Cotes.</i>	2.000
D°. <i>Oxford Soc.</i>	1.979
A Gallypot. <i>J. C.</i>	1.928
Alabaster. <i>Ward. C.</i>	1.874
Do. <i>Oxford Soc.</i>	1.872
A spotted facitious Marble. <i>J. C.</i>	1.822
Stone Bottle. <i>Oxford Soc.</i>	1.777
A piece of a glass (perhaps glazed) coffee-dish of a brown colour. <i>Harris.</i>	1.766
Barrel Clay. <i>L.</i>	1.712
Lapis de Goa. <i>Davies.</i>	1.710
Lapis rufus Bremensis. <i>Muschenb.</i>	1.666

An Icicle broken from a Grotto (I suppose

Stalactites) *Dr. Slare*, in *Harris*. . 1.190

Chalk, as found by *Dr. Slare*. *Harris*. 1.079

The mean gravity of Stone appears to be to that of water as about two and a half to one, and many stones of great hardness, such as the Onyx, Turquoise, Agat, Marble, Flint &c. do not much exceed that weight. It may therefore well be doubted whether such Stones whose specific gravity comes up to near three times that of water, or even beyond it, owe their density to metalline additions; or whether they are really formed of a different species of matter, as the Diamond seems to be.

Coral by its density appears to be a stone, tho in a vegetating state: or it may possibly from some late observations, be of an animal nature.

What is called *Lapis Hibernicus*, is a soft stone containing Vitriol.

We have not many observations upon Earths: by those we have, it seems probable that they contain the same kind of matter in a lax form, of which Stones are a more solid and denser concretion.

Lapis de Goa is but a trifling composition, perhaps hardly worth retaining in the tables.

What species of body should *Alabaster* be accounted? which with a stone-like hardness, yet falls so much below other Stones, or even Earths, in gravity.

TABLE V.

Of Sulphurs and Bitumens.

SULPHUR. <i>Petitus.</i>	2.344
Do. a piece of roll. <i>Hauksbee.</i>	2.010
Do. vive. <i>Boyle.</i>	2.000
D ^o . German, very fine. <i>Boyle.</i>	1.980
Do. transparent, Persian. <i>Davies.</i>	1.950
Sulphur mineralis. <i>Freind.</i>	1.875
Brimstone, such as is commonly fold.	
<i>J. C.</i>	1.811
Do. <i>Cotes.</i>	1.800
Asphaltum. <i>Boyle.</i> III. 243. <i>a.</i>	1.400
Scotch Coal. <i>Boyle.</i> III. 243. <i>a.</i>	1.300
Coal, of Newcastle. <i>L.</i>	1.270
Do. Pit, of Staffordshire. <i>Oxford Soc.</i>	1.240
Jet. <i>J. C.</i>	1.238
Do. <i>Davies.</i>	1.160
Do. <i>Davies.</i>	1.020
Succinum citrinum. <i>Davies.</i>	1.110
Id. pingue. <i>J. C.</i>	1.087
Id. flavum (by 2 experiments). <i>Davies.</i>	1.080
Id. pellucidum. <i>J. C.</i>	1.065
Id. album, item pingue. <i>Davies.</i>	1.060
Amber. <i>Boyle. Newton. C.</i>	1.040
Fine Gunpowder. <i>Reynolds.</i>	0.698

Sulphur is in gravity very nearly the same as Earth, so that its purity can hardly be ascertained by its weight, unless the matter it is associated with is of a stony density.

The

The semidiaphanous *Sulphur* is a beautiful kind which I have but seldom seen: it is in lumps of the size of a small bean.

Coal, the sorts here taken notice of are considerably lighter than *Sulphur*: but there are many other kinds, and of different weights.

I take the *Gagates* or *Jet* to differ very little from the *Channel Coal*.

The different sorts of *Amber* may be observed not to differ considerably in their several gravities.

Sulphurs seem to be the lightest of all mineral bodies.

TABLE VI.

Of Gums, Resins, &c.

GUM Arabic. <i>Freind.</i>	1.430
D°. <i>Newton. C.</i>	1.375
Opium. <i>Freind.</i>	1.360
Gum Tragacanth. <i>Freind.</i>	1.330
Myrrh. <i>Freind.</i>	1.250
Gum Guaiac. <i>Freind.</i>	1.224
Resina Scammonii. <i>Freind.</i>	1.200
Aloes. J. C. (qu. whether the resin or the wood).	1.177
Afa fatida, a very fine sample. <i>Hauksbee.</i>	1.251
D°. from <i>Dr. John Keill's Introd. ad veram Physicam.</i>	1.143
Pitch. <i>Oxford Soc. C.</i>	1.150
Thus. <i>Freind.</i>	1.071
Camphire. <i>Newton. C.</i>	0.996
Bees-wax. <i>Cotes.</i>	0.955
	Cera.

Cera. <i>Ghetaldus</i> . (ad aquam ut 95 $\frac{1}{2}$ ad 100).	0.954
Wax well freed from the honey. <i>Davies</i> .	0.938
Cera. <i>Petitus</i> .	0.937
D ^o . the same lump 2 years after. <i>Davies</i> .	0.942
Balsamus de Tolu. <i>Muschenbr.</i>	0.896
Mastic. <i>J. C.</i> (qu. whether the gum or the wood).	0.849

The bees wax in my own experiments was well freed from honey, by the boiling it in water, which probably made it lighter than it was set down in Mr. *Cotes's* Table: and the second experiment which I made two years after the first, if the difference was not owing to the difference of heat, is an instance of what I take to be a pretty general truth, that bodies become more dense and compact by rest, and that they would also be found heavier in the scale, in those cases where they do not lose weight by the evaporation of humidity.

The weights of vegetable Gums nearly correspond with those of the ligneous parts.

TABLE VII.

Of Woods, Barks &c.

COCO Shell. <i>Boyle</i> .	1.345
Bois de Gayac. <i>Muschenbr.</i>	1.337
Lignum Guaiacum. <i>Freind</i> .	1.333
Lignum Vitæ. <i>Oxf. Soc.</i>	1.327
Speckled Wood of Virginia. <i>Oxf. Soc.</i>	1.343
Cortex Guaiaci. <i>Freind</i> .	1.250
Lignum	

Lignum Nephriticum. <i>Freind.</i>	1.200
Lignum Asphaltum. <i>J. C.</i>	1.179
Ebony. <i>J. C. Item Aloes. J. C.</i>	1.177
Santalum rubrum. <i>J. C.</i>	1.128
Id. album. <i>J. C.</i>	1.041
Id. Citrinum. <i>J. C.</i>	0.809
Lignum Rhodium. <i>J. C.</i>	1.125
Radix Chinæ. <i>Freind.</i>	1.071
Dry Mahogany. <i>L.</i>	1.063
Gallæ. <i>Freind.</i>	1.034
Red wood. <i>Oxf. Soc.</i> It. Box wood. <i>Oxf.</i>	
<i>Soc. Ward. C.</i>	1.031
Log wood. <i>Oxf. Soc.</i>	0.913
Oak, dry, but of a very sound close texture.	
<i>Oxf. Soc.</i>	0.932
D ^o . tried another time. <i>Oxf. Soc.</i>	0.929
D ^o . sound dry. <i>Ward.</i>	0.927
D ^o . dry. <i>Cotes.</i>	0.925
D ^o . dry, English. <i>L.</i>	0.905
Oak of the outside sappy part, fell'd a year since. <i>Oxf. Soc.</i>	0.870
D ^o . <i>Reynolds.</i>	0.801
D ^o . very dry, almost worm-eaten. <i>Oxf.</i>	
<i>Soc.</i>	0.753
Dry Wainscot. <i>L.</i>	0.747
Beech meanly dry. <i>Oxf. Soc.</i>	0.854
Mastic (qu. if the wood or gum). <i>J. C.</i>	0.849
Ash dry about the heart. <i>Oxf. Soc.</i>	0.845
D ^o . dry. <i>Cotes.</i>	0.800
D ^o . meanly dry, and of the outside lax part of the tree. <i>Oxf. Soc.</i>	0.734
Elm dry. <i>L.</i>	0.800
D ^o . <i>Reynolds.</i>	0.768
D ^o .	

D°. <i>Oxf. Soc. C.</i>	0.600
Rad. <i>Gentianæ. Freind.</i>	0.300
Cortex <i>Peruvianus. Freind.</i>	0.734
Crabtree meanly dry. <i>Oxf. Soc.</i>	0.765
Yew, of a knot or root 16 years old. <i>Oxf. Soc.</i>	0.760
Maple dry. <i>Oxf. Soc. C.</i>	0.755
Plumtree dry. <i>J. C.</i>	0.663
Fir, dry yellow. <i>L.</i>	0.657
Dry white Deal. <i>L.</i>	0.569
Lignum <i>Abietin. Freind.</i>	0.555
Fir dry. <i>Cotes.</i>	0.550
D°. <i>Oxf. Soc.</i>	0.546
Walnut tree dry. <i>Oxf. Soc.</i>	0.631
Cedar dry. <i>Oxf. Soc.</i>	0.613
Juniper wood dry. <i>J. C.</i>	0.556
Sassafras wood. <i>J. C.</i>	0.482
Cork. <i>Cotes.</i>	0.240
D°. <i>J. C.</i>	0.237

Dr. *Jurin* has observed in the *Phil. Trans.* N°. 369. that the substance of all wood is specifically heavier than water, so as to sink in it, after the air is extracted from the pores and air-vessels of the wood, by placing it in warm water under the receiver of an air-pump; or if an air-pump cannot be had, by letting the wood continue some time in boiling water over a fire. The several weights therefore above given must be looked upon as the weights of the concrete bodies, in the condition they were, before the Air was either forcibly got out, or the water driven into the small hollows: and both these considerations may have their use

as notwithstanding that the specific weights of the solid particles are truly heavier than water, we shall from the weights of the bodies as they are now compounded, be enabled to make some judgment of their porosity, so far as they may be penetrable by water or other fluids.

TABLE VIII.
Of Animal Parts.

MANATI Lapis. <i>Boyle.</i>	2.860
Do. another. <i>Boyle.</i>	2.330
Do. a fragment of. <i>Boyle.</i>	2.290
Do. <i>J. C.</i> another from Jamaica. <i>Boyle.</i>	2.270
Pearl, very fine Seed, oriental. <i>Boyle. V.</i>	
12. a.	2.750
Do. a large one, weighing 206 grains.	
<i>Boyle V. 7. b.</i>	2.510
Murex Shell. <i>J. C.</i>	2.590
Crabs Eyes artificial. <i>Boyle.</i>	2.480
Do. native. <i>Boyle.</i>	1.890
Os ovinum recens. <i>Freind.</i>	2.222
Oyster Shell. <i>J. C.</i>	2.092
Calculus humanus, just voided. <i>Davies.</i>	2.000
Do. <i>Boyle. V. 7. b.</i>	1.760
Do. <i>Boyle.</i>	1.720
Do. <i>Cotes.</i>	1.700
Do. <i>Boyle. V. 7. b.</i>	1.690
Do. <i>J. C.</i>	1.664
Do. <i>Davies.</i>	1.650
Do. <i>Boyle.</i>	1.470
Do.	

D°. <i>J. C.</i>	1.433
D°. <i>Davies.</i>	1.330
D°. <i>J. C.</i>	1.240
Rhinoceros Horn. <i>Boyle.</i>	1.990
The top part of one. <i>J. C.</i>	1.242
Ebur. <i>Freind.</i>	1.935
Ivory. <i>Boyle.</i>	1.917
D°. dry. <i>Oxford Soc. C.</i>	1.826
D°. <i>Ward.</i>	1.823
Unicorn's Horn, a piece. <i>Boyle.</i>	1.910
Cornu Cervi. <i>Freind.</i>	1.875
Ox's Horn, the top part of one. <i>J. C.</i>	1.840
Blade bone of an Ox. <i>J. C.</i>	1.656
A stone of the Bezoar kind found with four others in the intestines of a mare. <i>Edw. Bailey M.D. of Havant in Hampshire. See Philosoph. Transact. No. 481.</i>	
Bezoar stone. <i>Boyle.</i>	1.640
D°. a large one. <i>Davies.</i>	1.570
D°. being the kernel of another. <i>Boyle.</i>	
V. 8. a.	1.550
D°. a fine oriental one. <i>Boyle.</i>	1.530
D°. two weigh'd separately. <i>Davies.</i>	1.504
D°. <i>Cotes.</i>	1.500
D°. <i>Boyle.</i>	1.480
D°. <i>Boyle.</i>	1.340
A stone from the Gall-bladder. <i>Hales.</i>	1.220
Blood human, the globules of it. <i>Furin by calculation.</i>	1.126
D°. the Crassamentum of. <i>Furin from Experiments.</i>	1.086
D°. <i>Davies.</i>	1.084

Do. from another Experiment.	<i>Jurin.</i>	1.082
Sanguinis humani cuticula alba.	<i>Davies.</i>	1.056
Human blood when grown cold.	<i>Jurin.</i>	1.055
The same as running immediately from the vein.	<i>Jurin.</i>	1.053
The serum of human blood.	<i>Jurin.</i>	1.030
Do. <i>Davies.</i>		1.026
Ichthyocollo.	<i>Freind.</i>	1.111
A Hen's Egg.	<i>Davies.</i>	1.090
Milk.	<i>J. C. C.</i>	1.031
Lac caprinum.	<i>Musschenbr.</i>	1.009
Lac.	<i>Freind.</i>	0.960
Urine.	<i>J. C. C.</i>	1.030
Id.	<i>Freind.</i>	1.012

Manati Lapis is said to be a stone, found in the head of the Manatee, or Sea-Cow of the *West-Indies*. See *Ray's Synopsis methodica Animalium Quadrupedum &c. Lond. 1693. 8o*. These Stones and Pearls are the heaviest of all the animal productions we are acquainted with.

Dr. *Jurin* has observed, *Phil. Transf.* No. 369. that, in examining fresh Human *Calculi* whilst they were still impregnated with Urine, he had met such as exceeded the weight of some sorts of burnt earthen ware and alabaster, and approached very near to that of brick, and the softer sort of paving stone; which I have myself also found to be true. Whereas those who have made their experiments upon such *Calculi*, as had most probably been a considerable time taken out of the bladder, and had consequently lost much of their weight, by the evaporation of the urine, with which they had at first

first been saturated, have found those Stones commonly to have been but about one half part, and some of them no more than a fourth part, heavier than an equal bulk of Water. From whence it has been too hastily concluded, that these Stones have very improperly been called by that name, as not at all approaching to the Specific Gravity of even the lightest real stones that we have any account of.

The *Calculus Humanus* and *Animal Bezoar* approach nearly to each other in their Specific Gravity.

Mr. *Boyle* has taken notice of the great difference to be found between the gravity of the true and the factitious Crabs-eyes. It is strange that the factitious should be made of such materials as can bring them so near to the mean gravity of true stones: and this consideration may deserve the attention of those, who may think that any particular dependence is to be had upon the use of these bodies in medicine.

Dr. *Furin* was the first who carefully examined the Specific Gravities of the different parts which compose Human Blood; and his experiments were performed with the greatest accuracy. It may be observed, that the Blood is, by an easy *analysis* divided into *Serum* and *Craassamentum*; and the *Craassamentum* again into the Glutinous and the Red globular parts, whose Specific Gravities are the greatest. It had before these experiments been the general received opinion, that the globules of the Blood were lighter than the Serum; and this indeed seemed to follow from Mr. *Boyle's* Experiments in his Natural History of *Human Blood*; from which he deduced the Specific Gravity of the mass itself; to be to that
of

of Water as 1040 to 1000, and that of the Serum alone to be to the same as 1190. And these numbers 1040 and 1190 had accordingly, till Dr. *Furin* re-examined the affair, been constantly taken to represent the true gravities of Human Blood and its Serum respectively. See Dr. *Furin's dissertation in Phil. Trans.* N^o. 361.

Milk is made by Dr. *Freind* to fall more short of the Gravity of Water, than it is made to exceed the same by *J. C.* Possibly this difference might arise from the Milk's being taken in one case warm from the cow, and in the other after it had stood some time.

TABLE IX.

Of Salts.

MERCURIUS dulcis bis sublim. <i>Mussh.</i>	12.353
Mercurius dulcis. <i>Freind.</i>	11.715
Id. ter sublim. <i>Muschenbr.</i>	9.882
Id. tertio sublim. Item Panacea rubra. <i>Freind.</i>	9.372
Id. quater sublim. <i>Muschenbr.</i> Item Turpethum minerale.	8.235
Id. 4to sublim. Item Turpeth mineral. <i>Freind.</i>	7.810
Sublimat. corrosiv. <i>Muschenbr.</i>	8.000
Id. <i>Freind.</i>	6.045
Cinis clavellatus, sordibus faleque suo neutro quodam (quod fere semper magis vel minus in cinere illo reperitur) depurgatus. <i>Fahrenheit.</i>	3.112
Sal illud neutrum. <i>Fahrenheit.</i>	2.642
2	Saccharum

Saccharum Saturni. Item sal Nitri fix.	
<i>Muschenbr.</i>	2.745
Eadem. <i>Freind.</i>	2.600
Magisterium Coralli. Item Pulvis sympatheticus. <i>Freind.</i>	2.231
Tartarum vitriolatum. <i>Muschenbr.</i>	2.298
Id. <i>Freind.</i>	2.186
Sal mirabile Glauberi. <i>Muschenbr.</i>	2.246
Id. <i>Freind.</i>	2.132
Tartarum emeticum. <i>Muschenbr.</i>	2.246
Id. <i>Freind.</i>	2.077
Sal Gemmæ. <i>Newton. C.</i>	2.143
Nitrum. <i>Fahrenheit.</i>	2.150
Nitre. <i>Newton. C.</i>	1.900
Id. <i>Freind.</i>	1.671
Sal Guaiaci. Item Sal enixum. Item Sal prunellæ. Item S. Polychrest. <i>Muschenbr.</i>	2.148
Eadem omnia. <i>Freind.</i>	2.030
Sal maritimum. <i>Fahrenheit.</i>	2.125
Cremor Tartari. Item Vitriol. alb. Item Vitriol. rubefact. Item S. Vitriol. <i>Muschenbr.</i>	1.900
Cremor Tart. Item Vitriol. alb. <i>Freind.</i>	1.796
Vitriol English, a very fine piece. <i>Boyle.</i>	1.880
D°. Dantzick. <i>Newton. C.</i>	1.715
Alumen. <i>Fahrenheit.</i>	1.738
Alum. <i>Newton.</i>	1.714
Sal chalybis. <i>Freind.</i>	1.733
Borax. <i>J. C.</i>	1.720
D°. <i>Newton. C.</i>	1.714
Vitriolum viride. Item Calcanth. rubefact. Item S. Vitriol. alb. <i>Freind.</i>	1.671
Saccharum albis. <i>Fahrenheit.</i>	1.606 $\frac{1}{2}$
Mel. <i>Villalpandus.</i>	1.500
Id. <i>Ghetaldus</i> 1 $\frac{2}{3}$. Honey, <i>Cotes.</i>	1.450
Sal	

Sal volatile Cornu Cervi. <i>Muschenb.</i>	:	1.496
Id. <i>Freind.</i>	.	1.421
Sal Ammoniac. purum. Item Ens Martis		
semel sublimat. <i>Muschenb.</i>	.	1.453
Eadem. <i>Freind.</i>	.	1.374
Ens Martis ter sublimat. <i>Muschenb.</i>	.	1.269
Id. <i>Freind.</i>	.	1.233

Most of the experiments in the ninth table are taken from Dr. *Freind*, who weigh'd the Salts in Spirits of Wine, and register'd the proportional gravity of the Salts to the Spirits. But the misfortune is, that the gravity of the Spirits of Wine he made use of is not register'd: so that the experiments cannot with certainty be reduced to the common standard of Water. He has deliver'd the gravity of Spirits of Wine to be 0.818, and that of Spirits of Wine rectified to be 0.78. I have supposed the Salts to be weighed in the last, as being the fittest for the purpose: but which he really used can only be conjectured.

There appears indeed to be a way to discover the weight of the Spirits of Wine, in which Dr. *Freind* weighed his Salts: for he weighed 60 Grains of Mercury, both in Water and in Spirits of Wine, and the loss of its weight was respectively $4\frac{1}{4}$ Grains and $2\frac{3}{4}$. Now the gravities of these Fluids must be in the same proportion, and this would give for the weight of the Spirits of Wine 0.627, which is much too little for the weight of his own rectified Spirits tho even that is less than what is assigned by any other author. So that, upon the whole, nothing can really be concluded from this experiment; and it must

must be allowed besides, that 60 Grains of Mercury take up too small a bulk in these Fluids, to have their gravities determined with any exactness thereby.

As Professor *Musschenbroek* has given in his table the specific weights of many of the same salts which are mentioned by Dr. *Freind*, but which differ considerably from the weights above set down, as resulting from the Doctor's experiments, I have also transcribed the Professor's numbers from his own table. These do not however appear to me to be derived from new or differing experiments, but from the very same related by Dr. *Freind*, only computed from the supposition of a heavier sort of Spirits of Wine, whose specific gravity is supposed to have been 0.823. The gravity of the *Sublimate corrosive*, set down 8.000, I take to be a mistake, made by the writing down its comparative weight to that of the Spirits themselves, instead of the water to which it should have been referred.

It requires great care and attention to take the Specific Gravities of Salts with sufficient accuracy. They dissolve in Water, and in some degree in all Fluids that partake of the nature of Water. If therefore Spirits of Wine are made use of for this purpose, they ought to be highly rectified, their own gravity accurately ascertained, and their degree of heat should be preserved uniform. For as this Fluid rarefies much faster than Water does, a small difference of heat would sensibly affect the gravities of the Salts to be determined by it. And perhaps Spirit of Turpentine were a more proper Fluid to be employed on these occasions.

It is remarkable, that *Tartar vitriolat. Sal Gem. Sal mirabile, Sal maritimum, Nitre, &c.* being Salts composed of different Acids and an Alkaline Salt, should so far exceed in gravity the Vitriolic Salts, composed of the most heavy Acid and a metallic Earth. Is not this owing to its forming less solid Chrystals, and to its containing large quantities of Air concealed in its Pores?

The great difference in the weight of the *Nitre*, in the several experiments of *Fahrenheit, Newton*, and *Freind*, may possibly be owing to the quantity of its concealed Air.

T A B. X.

Of Fluids.

MERCURY. <i>Ward. C.</i> (See Tab. I. among the Metals.)	14.000
Oleum Vitrioli. <i>Fahrenheit.</i>	1.8775*
Oyl of Vitriol. <i>Newton. C.</i>	1.700
Spiritus Nitri Hermeticus. <i>Freind.</i>	1.760
Id. <i>Muschenb.</i>	1.610
Lixivium cineris clavellati, sale quantum fieri potuit impregnatum. <i>Fahrenheit.</i>	1.5713*
Id. alio tempore præparatum. <i>Fahrenb.</i>	1.5634*
Oil of Tartar. <i>Cotes. Ol. Tartari per deliquium. Muschenb.</i>	1.550
Spiritus Nitri, cum Ol. Vitrioli. <i>Freind.</i>	1.440
Id. <i>Muschenb.</i>	1.338
Spiritus Nitri communis. Item, Bezoardicus. <i>Freind.</i>	1.410
	Spirit

Spirit of Nitre. <i>Cotes.</i>	Item	Sp. Nit.	
Bezoardicus. <i>Muschenb.</i>	.	.	1.315
Sp. Nitri. <i>Fahrenheit.</i>	.	.	1.2935*
Sp. Nitri dulcis. <i>Muschenb.</i>	.	.	1.000
Aqua fortis melioris notæ. <i>Fahrenheit.</i>	.	.	1.409*
Eadem, duplex. <i>Freind.</i>	.	.	1.340
Aqua fortis. <i>Cotes.</i>	.	.	1.300
Eadem, simplex. <i>Freind.</i>	.	.	1.100
Solutio salis comm. in aqua saturata.			
<i>Davies.</i>	.	.	1.244
Eadem, 1 in aquæ 2,7 part. ponderis.			
<i>Davies.</i>	.	.	1.240
Eadem 1 in aquæ 3 part. <i>Davies.</i>	.	.	1.217
Eadem, 1 in aquæ 3 part. <i>Freind.</i>	.	.	1.146
Eadem, 1 in aquæ 12 part. <i>Davies.</i>	.	.	1.060
Soap Lees the strongest. <i>Jurin.</i>	.	.	1.200
D°. Capital. <i>Jurin.</i>	.	.	1.167
Spirit of Vitriol. <i>Freind.</i>	.	.	1.200
Spiritus Salis cum Ol. Vitriol. <i>Muschenb.</i>	.	.	1.154
Idem, &c. <i>Freind.</i>	.	.	1.146
Spirit of Salt. <i>Cotes.</i>	Sp. Salis marini.		
<i>Muschenb.</i>	.	.	1.130
Sp. Salis communis. <i>Freind.</i>	.	.	1.037
Sp. Salis dulcis. <i>Muschenb.</i>	.	.	0.951
Id. <i>Freind.</i>	.	.	0.890
Sp. Salis Ammoniaci succinat. Item, cum ciner. clavellat. <i>Freind.</i>	.	.	1.120
Sp. Salis Ammoniac. cum calce. <i>Muschenb.</i>	.	.	0.952
Idem cum calce viva. <i>Freind.</i>	.	.	0.890
Sp. Cornu Cervi non rectific. <i>Freind.</i>	.	.	1.073
Sp. Serici. <i>Muschenb.</i>	.	.	1.145
Sp. Urinæ. <i>Cotes.</i>	.	.	1.120
Solutio Salis enixi, 1 in aquæ 5 part.			
<i>Freind.</i>	.	.	1.100
	Q q q 2		Oleum

Oleum Sassafras.	<i>Muschenb.</i>	1.094
Decoction Gentianæ.	<i>Freind.</i>	1.080
Sp. Tartari.	<i>Freind. Muschenb.</i>	1.073
Decoction Bistortæ.	<i>Freind.</i>	1.073
Decoction Sarzæ.	It. Chinæ. <i>Freind.</i>	1.049
Decoction Ari.	It. Sp. Salis comm. <i>Freind.</i>	1.037
Oleum Cinnamomi.	<i>Muschenb.</i>	1.035
Ol. Caryophyllorum.	<i>Muschenb.</i>	1.034
Beer Vinegar.	<i>Oxf. Soc.</i>	1.034
Acetum Vini.	<i>Muschenb.</i>	1.011
Id. distillatum.	<i>Muschenb.</i>	0.994
Acetum.	<i>Freind.</i>	0.976
Sack.	<i>Oxf. Soc.</i>	1.033
Sp. Ambræ.	<i>Muschenb.</i>	1.031
Sea-Water.	<i>Cotes.</i>	1.030
D ^o . settled clear.	<i>Oxf. Soc. Ward.</i>	1.027
College plain Ale.	<i>Oxf. Soc.</i>	1.028
Solutio Aluminis, 1 in aquæ 5.33 part.		
Item Solutio Sal. Amm. purif. 1, et vitriol. alb. 1, in aquæ 5 part.	<i>Freind.</i>	1.024
Laudanum liquidum Sydenhami.	It. Panacea	
Opii.	<i>Freind.</i>	1.024
Decoction Cort. Peruv.	Item, Granatorum.	
	<i>Freind.</i>	1.024
Moil Cyder, not clear.	<i>Oxf. Soc.</i>	1.017
Aqua fluviatilis.	<i>Muschenb.</i>	1.009
Tinctura Aloes cum aqua.	Item, Decoction	
Santali rubri.	<i>Freind.</i>	1.000
Rain Water.	<i>Newton, Reynolds.</i>	Common
Water.	<i>Cotes.</i>	Common clear Water.
	<i>Ward.</i>	Pump Water.
	<i>Oxf. Soc. J. C.</i>	
Aqua.	<i>Ghetaldus.</i>	Aqua pluviatilis.
	<i>Fahrenheit, Muschenb. &c.</i>	1.000
		Aqua

Aqua vel Vinum.	<i>Villalpandus.</i>	1.000
Aqua putealis.	<i>Muschenb.</i>	0.999
Oleum Fœniculi.	<i>Muschenb.</i>	0.997
Oleum Anethi.	<i>Muschenb.</i>	0.994
Aqua distillata.	<i>Muschenb.</i>	0.993
Wine, Claret.	<i>Oxf. Soc.</i>	0.993
D°. red.	<i>Ward.</i>	0.992
Vinum.	<i>Petitus.</i>	0.984
Id. <i>Ghetaldus.</i> (ad aquam ut 98 $\frac{1}{3}$ ad 100.)		0.983
Id. Burgundicum.	<i>Muschenb.</i>	0.953
Oleum Sabinæ. It. Hyssopi.	<i>Muschenb.</i>	0.986
Ol. Ambra. It. Pulegii.	<i>Muschenb.</i>	0.978
Ol. Mentha. It. Cumini.	<i>Muschenb.</i>	0.975
Decoctio Sabinæ.	<i>Freind.</i>	0.960
Infusio Marrubii. It. Mentha. It. Absynth.		
<i>Freind.</i>		0.950
Ol. Nucis Moschatæ.	<i>Muschenb.</i>	0.948
Ol. Tanacetii.	<i>Muschenb.</i>	0.946
Ol. Origani. It. Carvi.	<i>Muschenb.</i>	0.940
Elixir Propr. cum Sale volat. It. Infusio		
Theæ.	<i>Freind.</i>	0.940
Ol. Spicæ.	<i>Muschenb.</i>	0.936
Ol. Rorismarini.	<i>Muschenb.</i>	0.934
Linseed Oyl.	<i>Newton. C.</i>	0.932
D°. <i>Ward.</i>		0.931
Spirits of Wine proof, or Brandy.	<i>Ward.</i>	0.927
Sp. of Wine well rectified.	<i>Newton. C.</i>	0.866
Alcohol Vini.	<i>Fahrenheit.</i>	0.826
Id. magis dephlegmatum.	<i>Fahrenheit.</i>	0.825
Sp. Vini.	<i>Freind.</i>	0.818
Id. rectific.	<i>Freind.</i>	0.781
Esprit de Vin etherè.	<i>Muschenb.</i>	0.732
	<i>Spiritus.</i>	

Spiritus Croci. <i>Freind.</i>	0.925
Lamp Oyl. <i>Reynolds.</i>	0.924
Oleum. <i>Ghetaldus.</i> (ad aquam ut 91 $\frac{2}{3}$ ad 100.)	0.916
Oyl Olive. <i>Newton. C.</i>	0.913
D ^o . <i>Ward.</i>	0.912
Sallad Oyl. <i>Reynolds.</i>	0.904
Oleum. <i>Villalpandus.</i>	0.900
Id. <i>Petitus.</i>	0.891
Ol. Raparum. <i>Fahrenheit.</i>	0.913
Id. It. Tinct. Chalyb. Mynsicht. It. Tinct. Sulphur cum Sp. Terebynth. <i>Freind.</i>	
It. Huile de semences de navets. <i>Musch.</i>	0.853
Sp. Mellis. <i>Musch.</i>	0.895
Sp. Salis Ammoniaci cum calce viva.	0.890
Oleum Aurantiorum. <i>Musch.</i>	0.888
Spirit of Turpentine. <i>Newton. C.</i>	0.874
Tinct. Castorei. Item Sp. Vini camphorat. <i>Freind.</i>	0.870
Oyl of Turpentine. <i>Boyle V. 22. a.</i>	0.864
Ol. Terebynth. <i>Freind.</i>	0.793
Ol. Ceræ. <i>Musch.</i>	0.831
Tinctura Corallii. <i>Freind.</i>	0.828
Aqua cocta. <i>Freind.</i>	0.750
Air. <i>Newton. C.</i>	0.00125
Aer <i>Princip. Edit. 3. p. 512.</i> Aer juxta superficiem terræ occupat quasi spatium 850 partibus majus quam aqua ejusdem ponderis.	0.00118
The same, by an experiment made by the late Mr. Francis Hauksbee F.R.S. when the barometer stood at 29.7 inches. See <i>Physico Mathem. Exp. pag. 74.</i>	0.00113

As to the absolute weight of water with which all the other bodies are compared in these Tables, Mr. Boyle tells us in his *Medicina Hydrostatica*, printed in the new Edition of his Works, V. 19. b. that he had found by his own experiments, that a cubic inch of clear water weighed 256 Troy Grains. And Mr. Ward of Chester, who afterwards pursued this affair with great accuracy, determined that a cubic inch of common clear water did weigh by his tryals 253.18 like Troy Grains, or 0.527458 decimals of the Troy Ounce, or 0.578697 of the Ounce Averdupois, agreeable to what Mr. Reynolds had formerly deliver'd, who found the inch cubic of Rain Water to weigh by his experiments 0.579036 decimals of the same Averdupois ounce, differing from the other only 0.000339 parts.

But, as the accuracy of all the experiments in these tables depends upon the identity of the weight of Common Water, it may not be improper to ascertain that point by a Note taken from Mr. Boyle's *Medicina Hydrostatica*, V. 18. b. where he expresses himself in the following manner.

—“ It speciously may, and probably will be
 “ objected, that — there may be a great disparity
 “ betwixt the liquors that are called, and that de-
 “ servedly, *Common Water*. And some travellers
 “ tell us from the press, that the water of a certain
 “ eastern river, which if I mistake not is *Ganges*,
 “ is by a fifth part lighter than our water. But—
 “ having had upon several occasions the opportunity
 “ as well as curiosity to examine the weight of
 “ divers waters, some of them taken up in places very
 “ distant

“ distant from one another. I found the difference
 “ between their specific gravities far less than almost
 “ any body would expect. And if I be not much
 “ deceived by my memory (which I must have
 “ recourse to, because I have not by me the notes
 “ I took of those trials) the difference between
 “ waters, where one would expect a notable dispa-
 “ rity, was but about the thousandth part (and
 “ sometimes perchance very far less) of the weight
 “ of either. Nor did I find any difference con-
 “ siderable in reference to our question, between
 “ the weight of divers waters of different kinds, as
 “ spring-water, river-water, rain-water, and snow-
 “ water; though this last was somewhat lighter
 “ than any of the rest. And having had the curio-
 “ sity to procure some water brought into *England*,
 “ if I much misremember not, from the river
 “ *Ganges* itself; I found it very little, if at all,
 “ lighter than some of our common waters.”

The heaviest fluid we are acquainted with, next
 to *Mercury*, is *Oyl of Vitriol*, or water impregnated
 with the *Vitriolic Acid* in the highest degree we
 can obtain it, being almost double the weight of
Water.

The next is probably the *saturated solution* of
 the *fix'd Salt of Vegetables*; being a ponderous
 Salt, and dissolving freely in *Water*.

The next to this is *Spirit of Nitre*. *Spirit of*
Salt is lighter, and inferior in weight to the *satu-*
rated solution of *Salt* itself.

It is observable, that *marine* or *common Salt*
 and *Nitre* differ little in gravity, contrary to the
 nature of their *Spirits*.

The

The several *solutions* of *common Salt*, if accurately repeated, would shew in what proportion the gravities of fluids increase, upon the addition of Salt: and that *Sea-Water* does not contain one twenty-fourth part of Salt.

I have omitted in this table the three animal fluids, Milk, Serum of Blood, and Urine, as the same may be seen before in the 8th table, that of *animal parts*; but it may be noted in general that the specific gravity of all these fluids is nearly the same as that of Sea Water.

There are in Dr. *Freind's* table several decoctions of Plants, which I have inserted, altho' they are not I think of much use, nor greatly to be depended upon. Several of them are lighter than common Water, in contradiction to Dr. *Jurin's* observation, that *Vegetable Parts* are all heavier than Water: But it is probable these Experiments were made before the *Decoctions* were reduced to the temper of *Common Water*.

What is meant by the *Aqua cocta* of Dr. *Freind* in his table, I cannot imagine; not having any idea of such a change by boiling or otherwise, as can deprive common water of a full fourth part of its weight.

Since the density of the Air is as the force by which it is compressed, it follows that the weight of any portion of Air must vary in the same proportion with the weight of the whole *Atmosphere*: which in our climate is not less than one tenth of the whole weight, allowing the *Barometer* to vary from 28 to 31 Inches.

Again, by an experiment of the late Mr. *Hauksbee's* in his *Phys. Mechan. exp.* pag. 170. the density of the air varies one eighth part between the greatest degree of Heat in Summer, and that of Cold in the Winter Season. So that the Air, in a hard frost when the *Mercury* stands at 31 inches, is near a fifth part specifically heavier, than it is in a hot day when the *Mercury* stands at 28 inches.

T A B. XI.

From Mons. Homberg and John Caspar Eifenschmid, of the proportion of the specific weights of certain fluids in the Winter to the weights of the same in the Summer Season.

Mercurius	.	.	.	1.00479
Aqua pluvialis	.	.	.	1.00809
Aqua fluviatilis	.	.	.	1.00811
Aqua distillata	.	.	.	1.00815
Spirit. Vitriol.	.	.	.	1.01272
Lac bubulum	.	.	.	1.01316
Aqua marina	.	.	.	1.01351
Spir. Salis	.	.	.	1.01467
Acetum	.	.	.	1.01600
Ol. Vitrioli	.	.	.	1.02131
Ol. Terebynth.	.	.	.	1.02141
Aqua fortis	.	.	.	1.02637
Ol. Tartari	.	.	.	1.03013
Spir. Vini	.	.	.	1.03125
Spir. Nitri	.	.	.	1.04386

The

The Oyls of Olive and sweet Almonds congealing with the cold, could not be examin'd by the *Aræometer* in the winter season.

According to this table, the increase of the specific weight of common water in the winter above its weight in the summer, is not more than about the one hundred and twenty-fourth part of the whole; which is little more than half of what Professor *Muschenbroek* has elsewhere accounted the same, *desorte qu'un pied cubique Rhenan d'Eau, qui pese environ 64 livres en Etè, se trouvera etre en Hiver de presque 65 livres. Essai de Physique p. 424.* but sure this difference is much too great.

Notwithstanding that all fluids are condensed by cold, it is only till such time as they are ready to freeze; for upon the freezing they immediately expand again, so as for the ice to be lighter specifically than the fluid of which it is formed, and to swim in it: *Muschenbroek* gives the specific weight of Ice to be to that of Water commonly as 8 to 9. *La pesanteur de la Glace est ordinairement a celle de l'Eau, comme 8 a 9. pag. 441.* I am not acquainted with any other accurate experiments upon this subject, and it is hard to get ice in which there are not large bubbles of air included.

The *Philosophical Society* at *Oxford*, together with their Table of *Specific Gravity* already so often mentioned in the foregoing pages, communicated besides at the same time, to the *Royal Society*, another Table of a grosser nature indeed, but which being printed in the same Number 169. of the *Philosophical Transactions*, and appearing to be of use for many purposes: I have thought

the same not improper to be here also transcribed.

*Of the weight of a cubic foot of divers grains
&c, tried in a vessel of well-season'd Oak,
whose concave was an exact cubic foot.*

The following bodies were poured gently into the vessel, and those in the first 12 experiments were weigh'd in scales turning with two ounces; but the last 7 were weighed in scales turning with one ounce. The pounds and ounces here mentioned are Averdupois weight.

	lb	3
1. A foot of <i>Wheat</i> (worth 6 s. a bushel).	47	8
2. <i>Wheat</i> of the best sort (worth 6 s. 4 d. a bushel). Both sorts were red <i>Lammas Wheat</i> of last year.	48	4
3. The same sort of <i>Wheat</i> measured a second time.	48	2
4. White <i>Oats</i> of the last year.	29	8
The best sort of <i>Oats</i> were 2 d in a bushel better than these.		
5. Blew <i>Pease</i> (of the last year) and much worm-eaten.	49	12
6. White <i>Pease</i> of the last year but one	50	8
7. <i>Barley</i> of the last year (the best sort sells for 1 s. 6 d. in a quarter more than this)	41	2
8. <i>Malt</i> of the last year's <i>Barley</i> , made 2 months before.	30	4
9. Field <i>Beans</i> of the last year but one.	50	8
	10.	

	lb	3
10. Wheaten <i>Meal</i> (unsifted).	31	0
11. Rye <i>Meal</i> (unsifted).	28	4
12. Pump <i>Water</i> .	62	8
13. Bay <i>Salt</i> .	54	1
14. White Sea <i>Salt</i> .	43	12
15. <i>Sand</i> .	85	4
16. Newcastle <i>Coal</i> .	67	12
17. Pit <i>Coal</i> , from <i>Wednesbury</i> 63; but this is very uncertain in the filling the interstices betwixt the greater pieces.	63	0
18. <i>Gravel</i> .	109	5
19. Wood <i>Asbes</i> .	58	5

Of the same nature is also the following account
of *The difference of the weight of some Liquors
upon the Tunn compared to Rain Water*, from the
Experiments made formerly by Mr. *Reynolds* in the
Tower of London, and communicated to the *Royal
Society*, with his others before-mentioned, by Mr.
Smethwick, July 7. 1670.

	lb	3	<i>Averdupois</i>
Muscadine Wine was found heavier than Rain Water	11	2	
Milk	8	4	
Sherry	5	3	
Ale	5	2	
Canary Wine	3	3	
Small Beer	1	3	

White Wine was found lighter than
Rain Water

1 2

Rhenish

	lb	3	<i>Averdupois</i>
Rhenish Wine	1	4	
Claret	1	6	
Sallet Oyl	21	6	

The proportion given by this Author as the true one of the *Averdupois* Pound to the *Troy* Pound is, that fourteen of the former are equal to seventeen of the latter.

From whence the *Averdupois* Pound would be found equal to 6994.285, and the *Ounce* to 437.143 *Troy Grains*; which is indeed a little less than the same have since been determined by others; for Mr. *Ward* of *Chester* gives from a very nice experiment as he calls it, of his own, that one pound *Averdupois* was equal to 14 ounces 11 pennyweight and $14\frac{1}{2}$ *Troy Grains*, or to 6999 $\frac{1}{2}$, and consequently the ounce *Averdupois* to 437.47 of the same grains. And several Gentlemen of the *Royal Society*, who very carefully on 22 *April* 1743. examined the original standards of weights kept in the *Chamberlain's Office* of his MAJESTY'S *Exchequer*, found, upon the medium of the several trials which they made with those standards, that the *Pound Averdupeis* was equal to 7000.14, and the *Ounce Averdupeis* to 437.51 *Troy Grains*. *Phil. Trans* N°. 470.

I shall conclude these papers with the two Tables from *Marinus Ghetaldus* mentioned in the beginning, which I here transcribe, with an account of some of their uses, in his own words.

*Ad comparandum inter se duodecim corporum genera,
gravitate, et magnitudine Tabella.*

	Aurum	Arg. vivum	Plumb.	Argent.	Æs	Ferrum	Stann.	Mel	Aqua	Vinum	Cera	Oleum
Oleum	20 $\frac{8}{11}$	14 $\frac{62}{77}$	12 $\frac{6}{11}$	11 $\frac{3}{11}$	9 $\frac{9}{11}$	8 $\frac{8}{11}$	8 $\frac{4}{11}$	13 $\frac{2}{11}$	1 $\frac{1}{11}$	1 $\frac{4}{11}$	1 $\frac{5}{11}$	1
Cera	19 $\frac{19}{21}$	14 $\frac{32}{147}$	12 $\frac{1}{21}$	10 $\frac{52}{63}$	9 $\frac{9}{21}$	8 $\frac{8}{21}$	7 $\frac{89}{105}$	11 $\frac{109}{1210}$	1 $\frac{1}{21}$	1 $\frac{12}{120}$	1	
Vinum	19 $\frac{19}{19}$	13 $\frac{321}{413}$	11 $\frac{41}{59}$	10 $\frac{30}{59}$	9 $\frac{9}{59}$	8 $\frac{8}{59}$	7 $\frac{31}{59}$	12 $\frac{8}{59}$	1 $\frac{1}{59}$	1		
Aqua	19	13 $\frac{4}{7}$	11 $\frac{1}{2}$	10 $\frac{1}{3}$	9	8	7 $\frac{2}{3}$	12 $\frac{9}{20}$	1			
Mel	13 $\frac{3}{29}$	9 $\frac{73}{203}$	7 $\frac{27}{99}$	7 $\frac{11}{87}$	6 $\frac{6}{29}$	5 $\frac{15}{29}$	5 $\frac{3}{29}$	1				
Stannum	23 $\frac{1}{37}$	12 $\frac{21}{39}$	11 $\frac{41}{74}$	11 $\frac{44}{111}$	13 $\frac{8}{37}$	13 $\frac{1}{37}$	1					
Ferrum	23 $\frac{3}{8}$	13 $\frac{9}{79}$	11 $\frac{7}{16}$	12 $\frac{4}{24}$	13 $\frac{1}{8}$	1						
Æs	21	16 $\frac{3}{2}$	11 $\frac{5}{18}$	12 $\frac{4}{27}$	1							
Argentum	13 $\frac{6}{31}$	12 $\frac{68}{117}$	16 $\frac{7}{2}$	1								
Plumbum	11 $\frac{15}{23}$	11 $\frac{29}{161}$	1									
Arg. viv.	13 $\frac{38}{91}$	1										
Aurum	1											

Quero,

Quæro, exempli gratia, quam habet rationem in gravitate plumbum ad aurum. Intelligatur plumbum, quoniam levius est auro, gravitatem habere 1, et in linea plumbi, in prima columna nominata, sub titulo auri, quæraturs auri gravitas, ea erit $1\frac{1}{2}\frac{5}{3}$. Plumbum igitur ad aurum rationem habebit in gravitate ut 1, ad $1\frac{1}{2}\frac{5}{3}$. Si enim sumantur duo corpora magnitudine equalia, unum plumbeum alterum aureum, sit autem plumbei corporis gravitas 1, aurei erit $1\frac{1}{2}\frac{5}{3}$; quare corpus plumbeum ad corpus aureum ejusdem magnitudinis rationem habebit in gravitate ut 1, ad $1\frac{1}{2}\frac{5}{3}$. Comparantur autem inter se genera diversa gravitate, in corporibus magnitudine equalibus.

Rursus, quæro quam habet rationem in gravitate aqua ad argentum vivum. Intelligatur aqua, ut levior argento vivo gravitatem habere 1, et in linea aquæ, sub titulo argenti vivi, quæraturs argenti vivi gravitas, ea erit $13\frac{4}{7}$; aqua igitur ad argentum vivum rationem habebit in gravitate ut 1, ad $13\frac{4}{7}$.

Contra, quæro quomodo se habent in magnitudine aurum et plumbum. Intelligatur aurum, quoniam gravius est plumbo, magnitudinem habere 1, et in linea plumbi, sub titulo auri, quæraturs plumbi magnitudo, ea erit $1\frac{1}{2}\frac{5}{3}$; aurum igitur ad plumbum se habebit in magnitudine ut 1, ad $1\frac{1}{2}\frac{5}{3}$: si enim sumantur duo corpora æque gravia, unum aureum, alterum plumbeum, sit autem corporis aurei magnitudo 1, plumbei erit $1\frac{1}{2}\frac{5}{3}$; quare corpus aureum ad corpus plumbeum ejusdem gravitatis se habebit in magnitudine ut 1, ad $1\frac{1}{2}\frac{5}{3}$. Comparantur autem inter se genera diversa magnitudine, in corporibus æque gravibus.

Quæro

Quæro denique, quomodo se habent in magnitudine ferrum, et aqua, ponatur ferrum, ut gravius aqua, magnitudinem habere 1, et in linea aquæ, sub titulo ferri, quæraturs aquæ magnitudo, ea erit 8, ferrum igitur ad aquam se habebit in magnitudine ut 1, ad 8.

Altera,

Altera, ad comparandum inter se duodecim corporum genera, gravitate, et magnitudine, Tabella.

	Oleum	Cera	Vinum	Aqua	Mel	beum.	Ferum	Æs	Argent.	Plum.	arg.viv	Aurum
Aurum	$4\frac{4}{7}$	$5\frac{5}{109}$	$5\frac{10}{77}$	$5\frac{5}{19}$	$7\frac{12}{190}$	$38\frac{18}{89}$	$42\frac{2}{19}$	$47\frac{7}{19}$	$54\frac{22}{57}$	$60\frac{10}{119}$	$71\frac{3}{7}$	100
Arg. viv.	$6\frac{4}{7}$	$7\frac{7}{109}$	$7\frac{14}{77}$	$7\frac{7}{19}$	$10\frac{13}{190}$	$54\frac{10}{89}$	$58\frac{8}{19}$	$66\frac{6}{19}$	$76\frac{8}{57}$	$84\frac{14}{119}$	100	
Plumbum	$7\frac{6}{7}$	$8\frac{76}{253}$	$8\frac{18}{69}$	$8\frac{16}{23}$	$12\frac{19}{23}$	$64\frac{8}{23}$	$69\frac{3}{23}$	$78\frac{6}{23}$	$89\frac{19}{69}$	100		
Argent.	$8\frac{3}{7}$	$9\frac{81}{41}$	$9\frac{6}{11}$	$9\frac{21}{31}$	$14\frac{11}{31}$	$71\frac{0}{31}$	$77\frac{12}{31}$	$87\frac{3}{31}$	100			
Æs	$10\frac{5}{27}$	$10\frac{20}{23}$	$10\frac{25}{27}$	$11\frac{1}{19}$	$16\frac{1}{19}$	$82\frac{2}{9}$	$88\frac{8}{9}$	100				
Ferum	$11\frac{1}{24}$	$11\frac{41}{44}$	$12\frac{7}{24}$	$12\frac{1}{12}$	$18\frac{1}{8}$	$92\frac{1}{2}$	100					
Stannum	$12\frac{43}{111}$	$12\frac{266}{407}$	$13\frac{32}{111}$	$13\frac{19}{37}$	$19\frac{27}{37}$	100						
Mel	$63\frac{19}{87}$	$65\frac{265}{319}$	$67\frac{71}{87}$	$68\frac{28}{29}$	100							
Aqua	$91\frac{2}{3}$	$95\frac{5}{11}$	$98\frac{1}{3}$	100								
Vinum	$93\frac{13}{59}$	$97\frac{47}{69}$	100									
Cera	$96\frac{2}{63}$	100										
Oleum	100											

Quæro exempli gratia, quænam sit ratio in gravitate, auri ad argentum. Intelligatur aurum quoniam gravius est argento, gravitatem habere 100, et in linea auri, sub titulo argenti, reperietur argenti gravitas $54\frac{2}{3}$, aurum igitur ad argentum rationem habebit in gravitate ut 100, ad $54\frac{2}{3}$. Si enim sumantur duo corpora, magnitudine æqualia, unum aureum, alterum argenteum, sit autem aurei corporis gravitas 100, erit argentei $54\frac{2}{3}$; quare corpus aureum ad corpus argenteum ejusdem magnitudinis, rationem habebit in gravitate, ut 100, ad $54\frac{2}{3}$.

Quæro, quomodo se habet in gravitate aqua ad vinum; quoniam aqua gravior est vino, intelligatur ejus gravitas 100, et quoniam in linea aquæ, sub titulo vini, datur vini gravitas $98\frac{1}{2}$, aqua ad vinum se habebit in gravitate, ut 100, ad $98\frac{1}{2}$.

Contra quæro quomodo se habent in magnitudine argentum, et aurum. Intelligatur argentum ut levius auro, magnitudinem habere 100, et in linea auri, sub titulo argenti, quærat auri magnitudo, ea erit $54\frac{2}{3}$, argentum igitur ad aurum se habebit in magnitudine, ut 100, ad $54\frac{2}{3}$. Si enim sumantur duo corpora æque gravia, unum argenteum, alterum aureum, sit autem argentei corporis magnitudo 100, erit aurei $54\frac{2}{3}$; quare corpus argenteum, ad corpus aureum ejusdem gravitatis, se habebit in magnitudine, ut 100, ad $54\frac{2}{3}$.

Quæro denique, quomodo se habent in magnitudine aqua et argentum vivum. Quoniam aqua levior est argento vivo, intelligatur ejus magnitudo 100, et in linea argenti vivi, sub titulo aquæ, quærat argenti vivi magnitudo, et reperietur $7\frac{1}{2}$, aqua igitur ad argentum vivum se habebit in magnitudine, ut 100, ad $7\frac{1}{2}$.

F I N I S.

E R R A T A:

N^o. 487. *in the Contents, Art. VII. and p. 320,*
Tit. VII. The Rev. Charles Lyttleton LL.D.
F. R. S. and for Archdeacon of Exeter, read
Dean of Exeter.

Printed for C. DAVIS, over-against *Gray's Inn Gate*
in Holbourn, PRINTER to the ROYAL SOCIETY,
M.DCC.XLIX.

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Printed for C. DAVIS, over-against *Gray's Inn Gate*
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MDCCLXIX.

I. *An Account of the Experiments made by some Gentlemen of the Royal Society, in order to measure the absolute Velocity of Electricity; communicated to the Royal Society by Mr. W. Watson F.R. S.*

Read Oct. 27. 1748. **I** LAID before the *Royal Society* the Beginning of last Winter an Account * of

what had been done by some Gentlemen, in order to ascertain the respective Velocities of Electricity and Sound; from which it appeared, that through a Space measuring 6732 Feet, the Electricity was perceptible in a Quantity of Time less than $\frac{837}{10000}$ of a Second. But the Gentlemen concerned were desirous, if possible, of ascertaining the absolute Velocity of Electricity at a certain Distance; and a Method had been thought of, by which this might be determined with great Exactness.

Accordingly, *August 5. 1748.* there met at *Shooter's Hill* for this Purpose, the *President* of the *Royal Society*, the Rev. Mr. *Birch*, the Rev. Dr. *Bradley*, Astronomer Royal, *James Burrow Esq;* Mr. *Ellicot*, Mr. *George Graham*, *Richard Graham Esq;* the Rev. Mr. *Lawrie*, *Charles Stanhope Esq;* and myself, who were of the *Royal Society*, Dr. *Bevis*, and Mr. *Grifchow jun.* a Member of the *Royal Academy of Sciences at Berlin.*

It was agreed to make the electrical Circuit of two Miles; in the middle of which an Observer was to take in each Hand one of the Extremities of

T t t

* See these *Transf.* N°. 485.

a Wire, which was a Mile in Length. These Wires were to be so disposed, that this Observer being placed upon the Floor of the Room near the electrical Machine; the other Observers might be able in the same View to see the Explosion of the charged Phial, and the Observer holding the Wires; and might take notice of the Time lapsed between the discharging the Phial and the convulsive Motions of the Arms of the Observer in consequence thereof; inasmuch as this Time would shew the Velocity of Electricity; through a Space equal to the Length of the Wire between the coated Phial and this Observer.

The electrifying Machine was placed in the same House as it was last Year. We then found ourselves greatly embarrassed by the Wire's being conducted by the Side of the Road, which we were compell'd to, on account of the Space necessary for the measuring of Sound: But so great a Distance from the Machine was not now wanted, though the Circuit through the Wire was intended to be at least two Miles. We had discover'd, by our former Experiments, that the only Caution now necessary was, that the Wires conducted upon dry Sticks should not touch the Ground, each other, or any Non-electric, in a considerable Degree, in any Part of their Length: If they did not touch each other, the Returns of the Wire, be they ever so frequent, imported little, as the Wire had been found to conduct Electricity so much better than the Sticks. It was therefore thought proper to place these Sticks in a Field fifty Yards distant from the Machine. The Length of this Field being eleven Chains, or 726 Feet, eight Returns of the Wire from the Top
to

to the Bottom of the Field, made somewhat more than a Mile, and sixteen Returns more than two Miles, the Quantity of Wire intended for the Electricity to pass through to make the Experiment.

We had found last Year, that, upon discharging the electrified Phials, if two Observers made their Bodies Part of the Circuit, one of which grasped the leaden Coating of the Phial in one Hand, and held in his other one Extremity of the conducting Wire; and if the other Observer held the other Extremity of the conducting Wire in one Hand, and took in his other the short iron Rod with which the Explosion was made; upon this Explosion, I say, they were both shocked in the same Instant, which was that of the Explosion of the Phial. If therefore an Observer, making his Body Part of the Circuit, was shocked in the Instant of the Explosion of the charged Phial in the middle of the Wire, no Doubt would remain of the Velocity of Electricity being instantaneous through the Length of that whole Wire. But if, on the contrary, the Time between making the Explosion, and seeing the Convulsions in the Arms of the Observer holding the conducting Wires, was great enough to be measured, we then should be able to ascertain its Velocity to the Distance equal to half the Quantity of Wire employed only, let the Manner of the Electricity's discharging itself be what it would.

It has been a Question with some, who have consider'd this Subject, whether the Electricity, in completing the Circuit from the Matter contained in the Glass, passed either by the Wire in the Mouth to the Coating of the Glass, the contrary Way by

the Coating to the Wire in the Mouth, or otherwise directed itself both Ways at once? That the Electricity must pass off one of these three Ways, was certain, as the Explosion would not be complete, unless in the Instant thereof some Matter very non-electric communicated between the Wire in the Mouth, and the Coating of the Glass. Unless therefore the Observer was placed in the Centre of the conducting Wires, it might be objected, that the Experiment was not made with the Exactness necessary; because any Person, who was of Opinion that the Electricity directed itself from the Mouth of the Glass to the Coating, might object, if the Wire from the short iron Rod to the Observer was only half the Length of that between the Observer and the Coating of the Glass, that the Electricity, in the Time found, passed only through the short Wire, and *vice versa*. But if, as it was here thought proper, the Observer was placed in the Centre of the conducting Wire, let the Direction of the Electricity be what it would, no Difference could happen in the Result of the Experiments, if made with the necessary Caution; because, if the Effects in the middle and both Ends of the Wires were instantaneous, the Conclusion therefrom would be very obvious. To make the Experiment, the same Phial filled with Filings of Iron, and coated with Sheet-Lead, which was used last Year, was placed in the Window of the Room near the Machine, and was connected to the prime Conductor by a Piece of Wire. To the Coating of this Phial a Wire was fastened; which, being conducted upon dry Sticks to the before-mentioned Field, was carried in like manner to the Bottom;

tom ; and being conducted thus from the Bottom of the Field to the Top, and from the Top to the Bottom seven other times, returned again into the Room and was held in one Hand of an Observer near the Machine. From the other Hand of this Observer, another Wire, of the same Length with the former, was conducted in the same manner, and returned into the Room, and was fasten'd to the iron Rod with which the Explosion was made. The whole Length of the Wires, allowing ten Yards for their Turns round the Sticks, amounted to two Miles a Quarter and six Chains, or 12276 Feet.

As the Night preceding these Experiments had been very rainy, Care was taken, by silk Lines properly disposed, that the Wires in their Passage from the Window of the House might not touch the Wood thereof; lest, from the Moisture of this Wood, the electrical Circuit might be shortened.

When all Parts of the Apparatus were properly disposed, several Explosions of the charged Phial were made; and it was invariably seen, that the Observer holding in each Hand one of the Extremities of these Wires was convulsed in both his Arms in the Instant of making the Explosions.

Instead of one, four Men were then placed holding each other by the Hand near the Machine, the first of which held in his right Hand one Extremity of the Wire, and the last Man the other in his left. They were all seen convulsed in the Instant of the Explosion. Every one who felt it, complained of the Severity of the Shock.

It was then desired, by one of the Gentlemen concerned, that an Explosion should be made with the Observer holding only one of the Wires. This

was

was done accordingly; but the Observer felt nothing, the Phial discharging itself in a different manner to what it did before, on account of the Circuit's not being completed.

It was then tried, whether an Observer would be shocked upon the Discharge of the Phial, if the two Wires at their Extremities slightly touched each other, whilst an Observer at the same time held one of these about a Foot from their Ends in each of his Hands. Upon Trial he felt nothing, though the Phial exploded very quick, because the iron Wire conducted the Electricity better than the Body of the Observer.

It was then tried, whether or no, as the Ground was wet, if the Explosion was made with the Observer holding the Extremity of each Wire standing upon the Ground near the Window of the House, any Difference would arise in the Success of the Experiment. No Difference was found, the Observer being shocked in the Instant of the Explosion, as before, in both his Arms, and across his Breast.

Upon these Considerations we were fully satisfied, that through the whole Length of this Wire, being, as I mentioned before, twelve thousand two hundred and seventy-six Feet, the Velocity of Electricity was instantaneous.

As it was found last Year, we observed again, that although the electrical Commotions were very severe to those who held the Wires, the Report of the Explosion at the prime Conductor was little, in comparison of that which is heard when the Circuit is short. From whence it was conjectured, that the very loud Report, in the Experiment of *Leyden* is confined to a very short Circuit.

II. *An Account of double Fœtus's of Calves, by Mons^r le Cat, M.D. F.R.S. &c. dated at Rouen, August 20. 1748. N. S. Translated from the French by T. S. M. D. F. R. S.*

Read Oct. 27. 1748. **I** HAVE, since the Month of *January* 1735. been in Possession of a Child, born in our City of *Rouen*, which has two Heads, four Arms, four lower Extremities, and two Trunks united, and as it were blended together. About that time I published * a Description of the internal Parts of this Monster, which had but one Heart ; but I did not cause Draughts to be taken of those Parts : and it would now be a difficult Matter to have them drawn so as to exhibit a good Representation of the State in which they then were. This Negligence, through which I am deprived of those curious and instructive Figures, which this monstrous Birth would have afforded, made me wish for a like Opportunity, in some measure at least to make amends for that Fault. This Opportunity presented itself in *January* 1748. not in a human *Fœtus*, but in a Calf, which the Butchers of our Hospital cut out of a Cow.

The Description which I shall give of this Monster, will be the Explanation of the Figures that represent it.

Plate

* *Journal de Verdun*, for March 1735. p. 194.

TAB. I. *Fig. 1.*

The outward Surface of the double Calf is here exhibited, about one fourth of the natural Size.

Fig. 2.

The Integuments of the Breast being raised, there appears the Union and reciprocal Insertion of the pectoral Muscles of each Subject into one common *Linea alba*. None but the inmost Plans were attached to the Bones.

Fig. 3.

The Muscles being removed, one *Sternum*, common to both Subjects, appears in Sight.

There was a *Sternum* intirely fimilar to this, on the other or opposite Side.

TAB. II. *Fig. 4.*

A. The *Apex* of the Heart common to both.

B. The right Auricle of the Subject *B*.

C. The inferior *Aorta*.

D. The superior *Aorta* of the Subject *B*, from which issue the right Subclavian †, and the Carotids **.

E. The pulmonary Artery of the same Subject *B*.
P, Its Lungs.

FF, The superior *Venæ cavæ* of both Subjects.

ff, Their inferior *Venæ cavæ*.

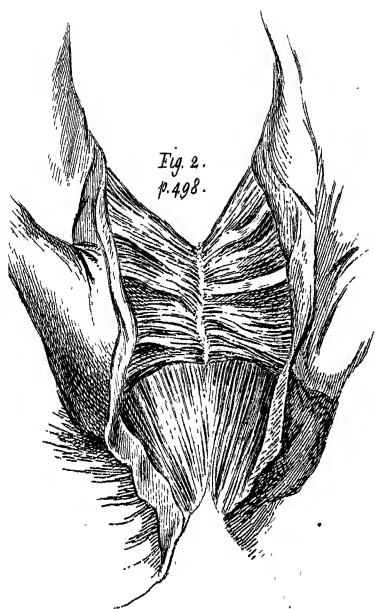
GG. The *Thymus*, or Throat Sweet-Breads.

α, of Subject *A*, A Trunk formed by the Reunion of the Carotids **, and the Subclavians †; which Trunk commonly constitutes the superior *Aorta*,

as

*Fig. 1.
p. 498.*

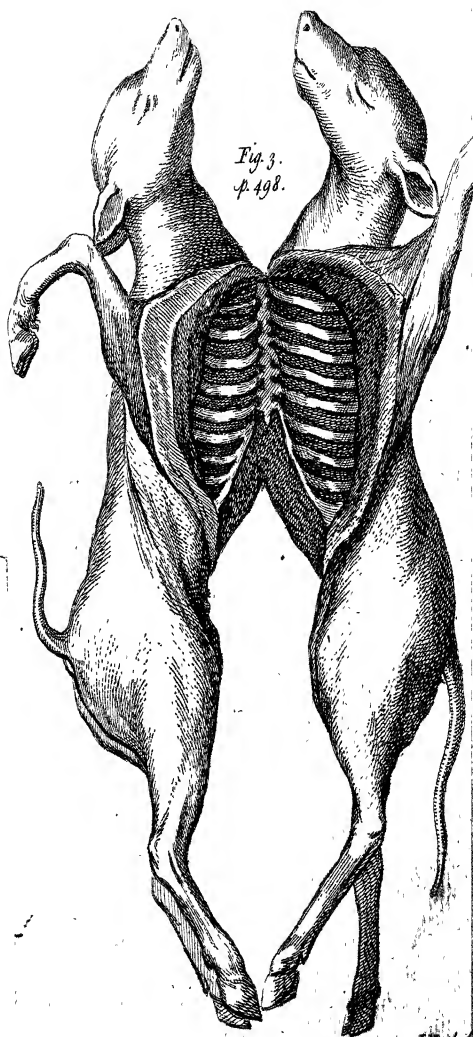




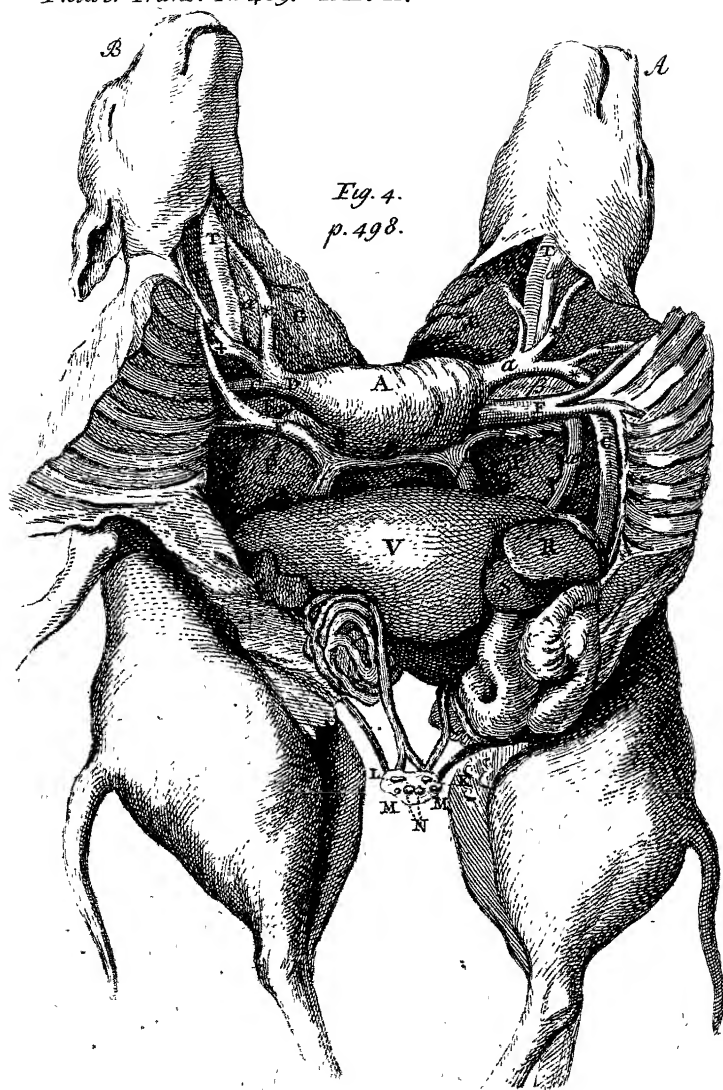
*Fig. 2.
p. 498.*



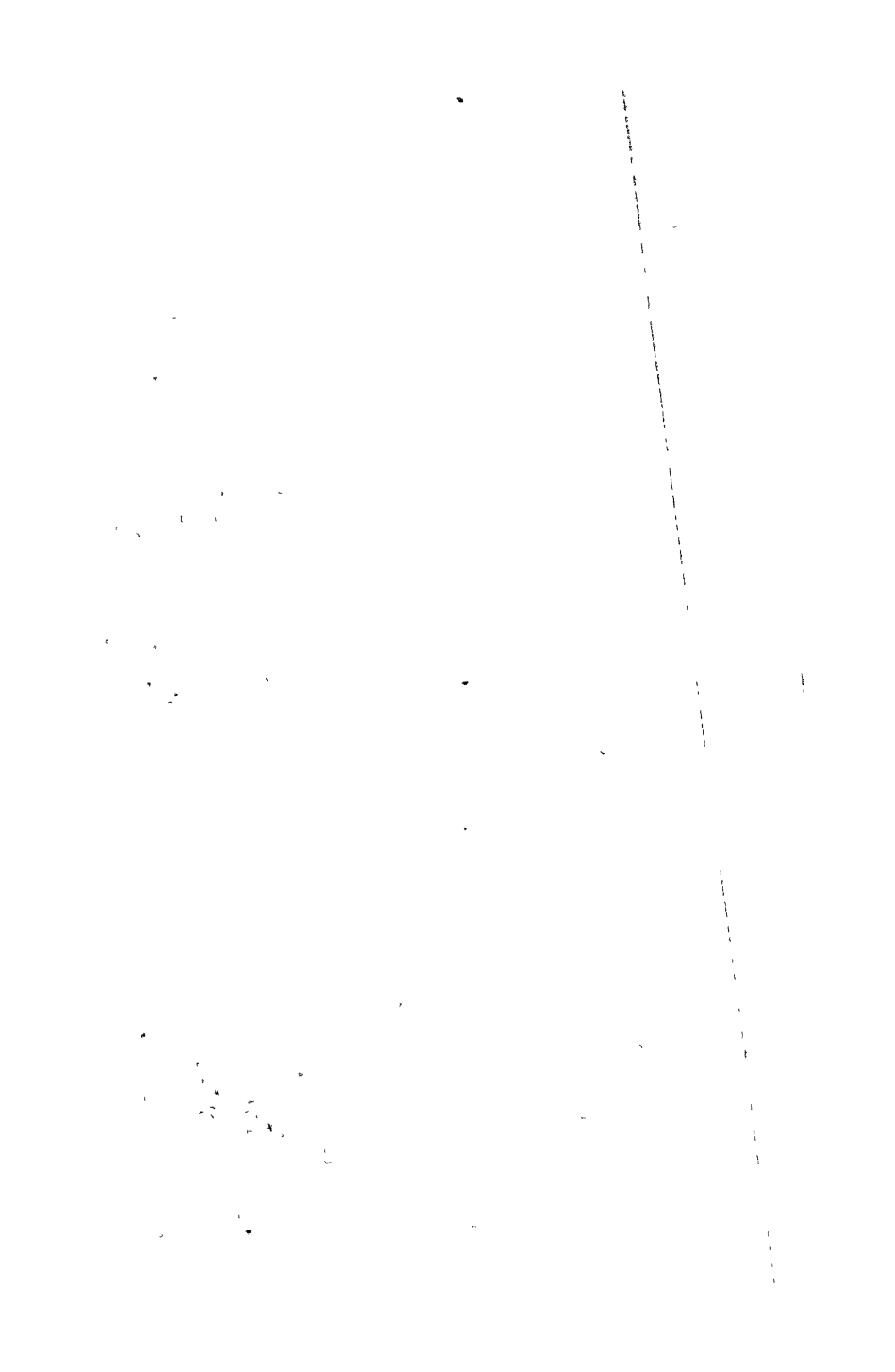
The piece of Lath. p. 521.



*Fig. 3.
p. 498.*







as appears in the Subject *B*: but in this Subject *A*, it only sends a small *Canalis arteriosus*, γ , into the inferior *Aorta C*.

β , A thick common Trunk of the pulmonary Artery and the inferior *Aorta*. The latter plainly appears a Continuation of this Trunk; whereas it is commonly a Continuation of the *Aorta*, α , *D*: and the pulmonary Artery, β , *E*, only furnishes the *Aorta*, α , γ , *C*, which makes but one Canal in ordinary Subjects, with a *Canalis arteriosus*, or Canal of Communication (See *Fig. 5.* in the Subject *B*). And indeed I am of Opinion, that this Structure, which seems extraordinary, is natural to every *Fœtus* that is not far advanced, as I explain it in my Course of Physiology under the Article of the *Fœtus*; and that it is a Consequence and Proof of the mechanical and successive Formation of the Organs of its Circulation, which begins by the lower Circle made by the umbilical Vein, as the first Mover; the Trunk of the *Vena cava*, the inferior *Aorta*, and the Branches of the *Vena cava*, which correspond with it. Now the Subject *A* had several Marks, which demonstrated that its Formation was less advanced than that of the Subject *B*.

a, a, a, The *Oesophagus* of each Subject. *TT*, The Windpipe.

R, The Spleen, *S*, the Stomach of the Subject *A*.

V, The Liver, which seems to belong to the Subject *B*.

Under them the great and small Intestines.

K, The umbilical Vein of the Subject *A*, through which having made an Injection, the whole Liver

U u u

was

was injected, but the Heart and Vessels were not: so that it is probable this Vein was distributed no farther than the Liver, and had no *Ductus venosus* that pass'd to the *Cava* and Heart.

L, The umbilical Vein of the Subject *B*, which received a large Branch of the umbilical Vein of the Subject *A*: and which Branch seem'd to supply the Place of the venal Duct, that was wanting.

Having thrown in the Injection through this Vein *L*, the Heart and Vessels of the two Subjects were injected.

MM, The Orifices of the umbilical Arteries, which were but two in Number, one for each Subject; the one and the other issuing from the right Iliac of each Subject.

N, The Openings of the *Urachi*, which were very large.

TAB. II. *Fig. 5.*

The other Side or View of this Monster, wherein the Subject *A* is to the right; the Subject *B* to the left.

a, The *Thymus* of each Subject.

b, The right Auricle of the Subject *A*.

c, The left Auricle of the Subject *B*.

d, The superior *Aorta* of the same.

e, Its pulmonary Artery.

f, The *Canalis arteriosus*, which here had the same Structure that all *Fœtus's* of nine Months or more usually have.

g, Its inferior *Aorta*.

hh, The inferior *Vena cavæ* of each Subject.

i, The

- i*, The *Azygos* of the Subject *B*.
k, Its *Oesophagus*.
l, Its Stomach.
m, Its Spleen.
nn, Part of the Lungs of each Subject.
o, The Liver of the Subject *A*.

TAB. II. *Fig. 6.*

This Plate represents the Heart, which is common to the two Subjects, laid open transversally. The anterior Part is lifted up, to shew the Cavities and the Orifices of the Vessels of its Base. The Figure is of about half the natural Size.

This Heart had only two Cavities, *D, E*, as usual; but the right Cavity or Ventricle *D* belonged to the Subject *B*; and the left Ventricle *E* to the Subject *A*.

Into each of the Cavities *DE* there opened four Orifices; *viz.* two arterial, which were those of the pulmonary Arteries, *a, a*, and of the *Aorta's*, *b, b*; and two venal Orifices, or those of the right and left Auricles, for the Blood of the *Cavæ*, *c, c*, and of the pulmonary Veins, *d, d*.

I give the Name of *Aorta* to the superior arterial Trunk *b*, of the Subject *A*, in Conformity with the usual Appellations, and because in common Subjects this Trunk alone deserves that Name; although in this Case the pulmonary Artery *a* visibly constitutes the principal Part of the inferior *Aorta*, *c*.

ff, The inferior *Cavæ*.

gg, The superior *Cavæ*.

h, Part of the Branches of the pulmonary Artery.

k, The Valve of the Orifice of the right Auricle in the Subject *B*.

III. *Extract of a Letter from the Rev. Dr. Doddridge to Henry Baker F. R. S. concerning a Wether giving Suck to a Lamb; and of a monstrous Lamb.*

Dear Sir,

Northampton, July 2. 1748.

Read Oct. 27.
1748.

THE Occasion of my writing to you now is to inform you of a remarkable Fact, which I have just heard from a Member of the Church of which I am Pastor, and in whom I can intirely confide. — He tells me, that he has in *Upper Heyford Field*, about four Miles from this Town, a Wether-Sheep which now suckles a Lamb. I know not by what Accident, the Lamb sometime since ran after it, and fixed upon its Paps; drawing hard, Milk followed. The Lamb has subsisted very well upon what it sucked from him, and at the late shearing Time he himself pressed the Teats, and Milk came out in a considerable Quantity.

This reminds me of what Mr. *Ray* tells us from *Boccone*, that a Countryman in *Umbria* nourished his Child by his own Milk, and *Florentinus* and *Malpighius* are quoted on the same Occasion. *Bartholinus*, in his *Anatomy*, p. 215. has some remarkable Passages to this Purpose: he quotes a Passage in *Aristotle* concerning a He-Goat in *Lemnos*, who had a great Quantity of Milk.

I shall add to this a short Account of a monstrous Lamb, which was weaned in a Field near *Newport Pagnel* about the Middle of last

last *March*, and was brought to me soon after it died. It had two perfect Heads (and two long Necks), each as large as that of a common Lamb, but sucked only with that on the right Side. So far as I could learn the Organs of both were complete. It walked only on four Legs, but had a fifth hanging down between the two Necks, rather longer than the other four; the Bones and Hoof were double, and had four Claws: the concave Side of it was turned upwards, and whenever the Creature walked this Leg moved up and down as it seemed spontaneously, and in a manner answerable to the Motion of the other four: it had two Tails, but no Vent behind: it had also two distinct Spines, but they met about five Inches above the Tail, and then divided again; but where they met they were not as one intire Spine, but as two adhering to each other. There were two Sets of Ribs, only those which met upward (where the Spine should regularly have been placed) were rather shorter than the other: and it seemed that the Blade-Bone belonging to the doubled Leg that grew between the Necks was larger than the rest, and seemed to be two Bones, but not intirely distinct: it had two Hearts of equal Bigness, lying over each other almost like a *St. Andrew's Cross*, or (as we should say in Heraldry) Saltire-wise. There were two *Oesophagi* and two *Asperæ Arteriæ*: four small Lobes of Lungs, but the two Gulletts were inserted into one common Stomach. — I am not Master enough of the Formation of ruminating Animals to inform you farther what was peculiar in this, We found nothing preternatural in the Formation of the Intestines, but the

the Tails grew so near, that the Return of both seemed to point to one Vent, tho' as I observed, the *Anus* was deficient. It had three Kidneys, one of them very large in proportion to the other two; so that I apprehend there was a Conjunction.

I fend you this Account whilst the Matter is fresh in my Memory, and am,

Dear Sir,

Yours, &c.

P. Doddridge.

IV. *Abstract from a Letter sent by Monsieur Buffon, Member of the Royal Academy of Sciences at Paris, &c. to Martin Folkes Esq; Pr. R. S. concerning his Re-invention of Archimedes's Burning Specula.*

Read Oct. 27.
1748.

AS what I read some time since to our *Royal Academy* upon the Subject of my Re-invention of *Archimedes's** burning *Specula*, cannot appear in our Memoirs before the Year 1747, I think of publishing by themselves my Observations upon these Mirrors, as soon as I shall satisfy myself upon certain Particulars, by some new Experiments I am now preparing to make. The *Speculum* I have already constructed, and which is but six Feet broad and as many high, burns Wood at the Distance of 200 Feet, it melts Tin and Lead
at

* See these *Trans.* No. 483.

at the Distance of above 120 Feet, and Silver at 50. The Theory which led me to this Discovery is founded upon two important Remarks, the one that the Heat is not proportional to the Quantity of Light, and the other that the Rays do not come parallel from the Sun. The first of those, which appears to be a Paradox, is nevertheless a Truth, of which one may easily satisfy one's self, by reflecting that Heat propagates itself even within Bodies; and that when one heats at the same time a large Superficies, the Firing is much quicker than when one only heats a small Portion of the same.

I am, &c.

*From the Chateau de Montbard in
Burgundy, Sept. 18. N.S. 1748.*

Buffon.

V. *An Essay on Quantity; occasioned by reading a Treatise, in which Simple and Compound Ratio's are applied to Virtue and Merit, by the Rev. Mr. Reid; communicated in a Letter from the Rev. Henry Miles D.D. & F.R.S. to Martin Folkes Esq; Pr.R.S.*

SECT. I.

What Quantity is:

Read Nov. 3. 1748. SINCE mathematical Demonstration is thought to carry a peculiar Evidence along with it, which leaves no Room for further Dispute; it may be of some Use, or Entertainment

tainment at least, to inquire to what Subjects this kind of Proof may be applied.

Mathematics contain properly the Doctrine of Measure; and the Object of this Science is commonly said to be Quantity; therefore Quantity ought to be defined, What may be measured. Those who have defined Quantity to be whatever is capable of More or Less, have given too wide a Notion of it, which I apprehend has led some Persons to apply mathematical Reasoning to Subjects that do not admit of it.

Pain and Pleasure admit of various Degrees, but who can pretend to measure them? Had this been possible, it is not to be doubted but we should have had as distinct Names for their various Degrees, as we have for Measures of Length or Capacity; and a Patient should have been able to describe the Quantity of his Pain, as well as the Time it began, or the Part it affected. To talk intelligibly of the Quantity of Pain, we should have some Standard to measure it by; some known Degree of it so well ascertained, that all Men, when they talked of it, should mean the same thing; we should also be able to compare other Degrees of Pain with this, so as to perceive distinctly, not only whether they exceed or fall short of it, but how far, or in what proportion; whether by an half, a fifth, or a tenth.

Whatever has Quantity, or is measurable, must be made up of Parts, which bear Proportion to one another, and to the Whole; so that it may be increased by Addition of like Parts, and diminished by Subtraction, may be multiplied and divided, and in a Word, may bear any Proportion to another

Quantity of the same kind, that one Line or Number can bear to another. That this is essential to all mathematical Quantity, is evident from the first Elements of Algebra, which treats of Quantity in general, or of those Relations and Properties which are common to all Kinds of Quantity. Every algebraical Quantity is supposed capable not only of being increased and diminished, but of being exactly doubled, tripled, halved, or of bearing any assignable Proportion to another Quantity of the same kind. This then is the Characteristic of Quantity; whatever has this Property may be adopted into Mathematics; and its Quantity and Relations may be measured with mathematical Accuracy and Certainty.

SECT. 2.

Of Proper and Improper Quantity.

There are some Quantities which may be called *Proper*, and others *Improper*. This Distinction is taken notice of by *Aristotle*; but it deserves some Explication.

I call that *Proper* Quantity which is measured by its own Kind; or which of its own Nature is capable of being doubled or tripled, without taking in any Quantity of a different Kind as a Measure of it. Thus a Line is measured by known Lines, as Inches, Feet, or Miles; and the Length of a Foot being known, there can be no Question about the Length of two Feet, or of any Part or Multiple of a Foot. And this known Length, by being multiplied or divided, is sufficient to give us a distinct Idea of any Length whatsoever.

Xxx

Improper

Improper Quantity is that which cannot be measured by its own Kind; but to which we assign a Measure by the means of some proper Quantity that is related to it. Thus Velocity of Motion, when we consider it by itself, cannot be measured. We may perceive one Body to move faster, another slower; but we can have no distinct Idea of a Proportion or *Ratio* between their Velocities, without taking in some Quantity of another Kind to measure them by. Having therefore observed, that by a greater Velocity a greater Space is passed over in the same time, by a less Velocity a less Space, and by an equal Velocity an equal Space; we hence learn to measure Velocity by the Space passed over in a given Time, and to reckon it to be in exact Proportion to that Space: And having once assigned this Measure to it, we can then, and not till then, conceive one Velocity to be exactly double, or half, or in any other Proportion to another; we may then introduce it into mathematical Reasoning without Danger of Confusion, or Error, and may also use it as a Measure of other Improper Quantities.

All the Kinds of Proper Quantity we know, may, I think, be reduced to these four, Extension, Duration, Number, and Proportion. Tho' Proportion be measurable in its own Nature, and therefore hath Proper Quantity, yet as Things cannot have Proportion which have not Quantity of some other Kind, it follows, that whatever has Quantity must have it in one or other of these three Kinds, Extension, Duration, or Number. These are the Measures of themselves, and of all Things else that are measurable.

Number

Number is applicable to some things, to which it is not commonly applied by the Vulgar. Thus, by attentive Consideration, Lots and Chances of various Kinds appear to be made up of a determinate Number of Chances that are allowed to be equal; and by numbering these, the Values and Proportions of those which are compounded of them may be demonstrated.

Velocity, the Quantity of Motion, Density, Elasticity, the *Vis insita*, and *impressa*, the various Kinds of centripetal Forces, and different Orders of Fluxions, are all Improper Quantities; which therefore ought not to be admitted into Mathematics, without having a Measure of them assigned. The Measure of an improper Quantity ought always to be included in the Definition of it; for it is the giving it a Measure that makes it a proper Subject of mathematical Reasoning. If all Mathematicians had considered, this as carefully as Sir *Isaac Newton* appears to have done, some Labour had been saved both to themselves and to their Readers. That Great Man, whose clear and comprehensive Understanding appears, even in his Definitions, having frequent Occasion to treat of such improper Quantities, never fails to define them, so as to give a Measure of them, either in proper Quantities, or in such as had a known Measure. This may be seen in the Definitions prefixed to his *Princip. Phil. Nat. Math.*

It is not easy to say how many Kinds of improper Quantity, may in time, be introduced into Mathematics, or to what new Subjects Measures may be applied: But this I think we may conclude,

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that

that there is no Foundation in Nature for, nor can any valuable End be served by applying Measure to any, thing but what has these two Properties. First it must admit of Degrees of greater and less. Secondly, it must be associated with, or related to something that has proper Quantity, so as that when one is increased the other is increased, when one is diminished, the other is diminished also; and every Degree of the one must have a determinate Magnitude or Quantity of the other corresponding to it.

It sometimes happens, that we have Occasion to apply different Measures to the same thing. Centripetal Force, as defined by *Newton*, may be measured various Ways, he himself gives different Measures of it, and distinguishes them by different Names, as may be seen in the above-mentioned Definitions.

In reality, I conceive that the applying of Measures to things that properly have not Quantity, is only a Fiction or Artifice of the Mind, for enabling us to conceive more easily, and more distinctly to express and demonstrate, the Properties and Relations of those things that have real Quantity. The Propositions contained in the two first Books of *Newton's Principia* might perhaps be expressed and demonstrated, without those various Measures of Motion, and of centripetal and impressed Forces which he uses: But this would occasion such intricate and perplexed Circumlocutions, and such a tedious Length of Demonstrations as would fright any sober Person from attempting to read them.

SECT. 3. *Coroll. first.*

From the Nature of Quantity we may see what it is that gives Mathematics such Advantage over other Sciences, in Clearness and Certainty; namely, that Quantity admits of a much greater Variety of Relations than any other Subject of human Reasoning; and at the same time every Relation or Proportion of Quantities may by the Help of Lines and Numbers be so distinctly defined, as to be easily distinguished from all others, without any Danger of Mistake. Hence it is that we are able to trace its Relations through a long Process of Reasoning, and with a Perspicuity and Accuracy which we in vain expect in Subjects not capable of Mensuration.

Extended Quantities, such as Lines, Surfaces and Solids, besides what they have in common with all other Quantities, have this peculiar, That their Parts have a particular Place and Disposition among themselves: A Line may not only bear any assignable Proportion to another, in Length or Magnitude, but Lines of the same Length may vary in the Disposition of their Parts; one may be straight, another may be Part of a Curve of any Kind or Dimension, of which there is an endless Variety. The like may be said of Surfaces and Solids. So that extended Quantities admit of no less Variety with regard to their Form than with regard to their Magnitude: And as their various Forms may be exactly defined and measured, no less than their Magnitudes, hence it is that Geometry, which treats of extended Quantity, leads us into a much greater Compass and Variety of Reasoning than any other Branch
of

of Mathematics. Long Deductions in Algebra for the most part are made, not so much by a Train of Reasoning in the Mind, as by an artificial kind of Operation, which is built on a few very simple Principles: But in Geometry we may build one Proposition upon another, a third upon that, and so on, without ever coming to a Limit which we cannot exceed. The Properties of the more simple Figures can hardly be exhausted, much less those of the more complex ones.

SECT. 4. *Coroll. 2.*

It may I think be deduced from what hath been above said, That mathematical Evidence is an Evidence *sui generis*, not competent to any Proposition which does not express a Relation of Things measurable by Lines or Numbers. All proper Quantity may be measured by these, and improper Quantities must be measured by those that are proper.

There are many Things capable of More and Less, which perhaps are not capable of Mensuration. Tastes, Smells, the Sensations of Heat and Cold, Beauty, Pleasure, all the Affections and Appetites of the Mind, Wisdom, Folly, and most Kinds of Probability, with many other Things too tedious to enumerate, admit of Degrees, but have not yet been reduced to Measure, nor, as I apprehend, ever can be. I say, most Kinds of Probability, because one Kind of it, *viz.* the Probability of Chances is properly measurable by Number, as is above observed.

Altho' Attempts have been made to apply mathematical Reasoning to some of these Things, and the Quantity of Virtue and Merit in Actions has been measured

measured by simple and compound *Ratio's*; yet I do not think that any real Knowledge has been struck out this Way: It may perhaps, if discretely used, be a Help to Discourse on these Subjects, by pleasing the Imagination, and illustrating what is already known; but until our Affections and Appetites shall themselves be reduced to Quantity, and exact Measures of their various Degrees be assigned, in vain shall we essay to measure Virtue and Merit by them. This is only to ring Changes upon Words, and to make a Shew of mathematical Reasoning, without advancing one Step in real Knowledge.

SECT. 5. *Coroll. 3.*

I apprehend the Account that hath been given of the Nature of proper and improper Quantity may also throw some Light upon the Controversy about the Force of moving Bodies, which long exercised the Pens of many Mathematicians, and for what I know is rather drop'd than ended; to the no small Scandal of Mathematics, which hath always boasted of a Degree of Evidence, inconsistent with Debates that can be brought to no Issue.

Tho' Philosophers on both Sides agree with one another, and with the Vulgar in this, That the Force of a moving Body is the same, while its Velocity is the same, is increased when its Velocity is increased, and diminished when that is diminished. But this vague Notion of Force, in which both Sides agree, tho' perhaps sufficient for common Discourse, yet is not sufficient to make it a Subject of mathematical Reasoning: In order to that, it must be more accurately defined, and so defined as to give
us

us a Measure of it, that we may understand what is meant by a double or a triple Force. The *Ratio* of one Force to another cannot be perceived but by a Measure; and that Measure must be settled not by mathematical Reasoning, but by a Definition. Let any one consider Force without relation to any other Quantity, and see whether he can conceive one Force exactly double to another; I am sure I cannot, nor shall, till I shall be endowed with some new Faculty; for I know nothing of Force but by its Effects, and therefore can measure it only by its Effects. Till Force then is defined, and by that Definition a Measure of it assigned, we fight in the dark about a vague Idea, which is not sufficiently determined to be admitted into any mathematical Proposition. And when such a Definition is given, the Controversy will presently be ended.

SECT. 6.

Of the Newtonian Measure of Force.

You say, the Force of a Body in Motion is as its Velocity: Either you mean to lay this down as a Definition as *Newton* himself has done; or you mean to affirm it as a Proposition capable of Proof. If you mean to lay it down as a Definition, it is no more than if you should say, I call that a double Force which gives a double Velocity to the same Body, a triple Force which gives a triple Velocity, and so on in Proportion. This I intirely agree to; no mathematical Definition of Force can be given that is more clear and simple, none that is more agreeable to the common Use of the Word in Language.

For,

For since all Men agree, that the Force of the Body being the same, the Velocity must also be the same; the Force being increased or diminished, the Velocity must be so also, what can be more natural or proper than to take the Velocity for the Measure of the Force?

Several other things might be advanced to shew that this Definition agrees best with the common popular Notion of the Word Force. If two Bodies meet directly with a Shock, which mutually destroys their Motion without producing any other sensible Effect, the Vulgar would pronounce, without Hesitation, that they met with equal Force; and so they do, according to the Measure of Force above laid down: For we find by Experience, that in this Case their Velocities are reciprocally as their Quantities of Matter. In Mechanics, where by a Machine two Powers or Weights are kept in *equilibrio*, the Vulgar would reckon that these Powers act with equal Force, and so by this Definition they do. The Power of Gravity being constant and uniform, any one would expect that it should give equal Degrees of Force to a Body in equal Times, and so by this Definition it does. So that this Definition is not only clear and simple, but it agrees best with the Use of the Word Force in common Language, and this I think is all that can be desired in a Definition.

But if you are not satisfied with laying it down as a Definition, that the Force of a Body is as its Velocity, but will needs prove it by Demonstration or Experiment; I must beg of you, before you take one Step in the Proof, to let me know what you

Y y y mean

mean by Force, and what by a double or a triple Force. This you must do by a Definition which contains a Measure of Force. Some primary Measure of Force must be taken for granted, or laid down by way of Definition; otherwise we can never reason about its Quantity. And why then may you not take the Velocity for the primary Measure as well as any other? You will find none that is more simple, more distinct, or more agreeable to the common Use of the Word Force: And he that rejects one Definition that has these Properties, has equal Right to reject any other. I say then, that it is impossible, by mathematical Reasoning or Experiment, to prove that the Force of a Body is as its Velocity, without taking for granted the thing you would prove, or something else that is no more evident than the thing to be proved.

SECT. 7.

Of the Leibnitzian Measure of Force.

Let us next hear the *Leibnitzian*, who says, that the Force of a Body is as the Square of its Velocity. If he lays this down as a Definition, I shall rather agree to it, than quarrel about Words, and for the future shall understand him, by a quadruple Force to mean that which gives a double Velocity, by 9 times the Force that which gives three times the Velocity, and so on in duplicate Proportion. While he keeps by his Definition, it will not necessarily lead him into any Error in Mathematics or Mechanics. For, however paradoxical his Conclusions may appear, however different in Words from theirs
who

who measure Force by the simple *Ratio* of the Velocity; they will in their Meaning be the same: Just as he who would call a Foot twenty-four Inches, without changing other Measures of Length, when he says a Yard contains a Foot and a half, means the very same as you do, when you say a Yard contains three Feet.

But tho' I allow this Measure of Force to be distinct, and cannot charge it with Falshood, for no Definition can be false, yet I say in the first place, it is less simple than the other; for why should a duplicate *Ratio* be used where the simple *Ratio* will do as well? In the next place, this Measure of Force is less agreeable to the common Use of the Word Force, as hath been shewn above; and this indeed is all that the many laboured Arguments and Experiments, brought to overturn it, do prove. This also is evident, from the Paradoxes into which it has led its Defenders.

We are next to consider the Pretences of the *Leibnitzian*, who will undertake to prove by Demonstration, or Experiment, that Force is as the Square of the Velocity. I ask him first, what he lays down for the first Measure of Force? The only Measure I remember to have been given by the Philosophers of that Side, and which seems first of all to have led *Leibnitz* into his Notion of Force, is this: The Height to which a Body is impell'd by any impressed Force, is, says he, the whole Effect of that Force, and therefore must be proportional to the Cause: But this Height is found to be as the Square of the Velocity which the Body had at the Beginning of its Motion.

In this Argument I apprehend that great Man has been extremely unfortunate. For, *1st*, Whereas all Proof should be taken from Principles that are common to both Sides, in order to prove a thing we deny, he assumes a Principle which we think farther from the Truth; namely, that the Height to which the Body rises is the whole Effect of the Impulse, and ought to be the whole Measure of it. *2^{dly}*, His Reasoning serves as well against him as for him: For may I not plead with as good Reason at least thus? The Velocity given by an impressed Force is the whole Effect of that impressed Force; and therefore the Force must be as the Velocity. *3^{dly}*, Supposing the Height to which the Body is raised to be the Measure of the Force, this Principle overturns the Conclusion he would establish by it, as well as that which he opposes. For, supposing the first Velocity of the Body to be still the same; the Height to which it rises will be increased, if the Power of Gravity is diminished; and diminished, if the Power of Gravity is increased. Bodies descend slower at the Equator, and faster towards the Poles, as is found by Experiments made on Pendulums. If then a Body is driven upwards at the Equator with a given Velocity, and the same Body is afterwards driven upwards at *Leipsick* with the same Velocity, the Height to which it rises in the former Case will be greater than in the latter; and therefore, according to his Reasoning, its Force was greater in the former Case; but the Velocity in both was the same; consequently the Force is not as the Square of the Velocity any more than as the Velocity.

Reflections on this Controversy.

Upon the whole, I cannot but think the Controversists on both Sides have had a very hard Task; the one to prove, by mathematical Reasoning and Experiment, what ought to be taken for granted, the other by the same means to prove what might be granted, making some Allowance for Impropriety of Expression, but can never be proved.

If some Mathematician should take it in his Head to affirm, that the Velocity of a Body is not as the Space it passes over in a given Time, but as the Square of that Space; you might bring mathematical Arguments and Experiments to confute him; but you would never by these force him to yield, if he was ingenuous in his Way; because you have no common Principles left you to argue from, and you differ from one another, not in a mathematical Proposition, but in a mathematical Definition.

Suppose a Philosopher has consider'd only that Measure of centripetal Force which is proportional to the Velocity generated by it in a given Time; and from this Measure deduces several Propositions. Another Philosopher in a distant Country, who has the same general Notion of centripetal Force, takes the Velocity generated by it, and the Quantity of Matter together, as the Measure of it. From this he deduces several Conclusions, that seem directly contrary to those of the other. Thereupon a serious Controversy is begun, whether centripetal Force be as the Velocity, or as the Velocity and Quantity of Matter taken together. Much mathematical and
 4 experimental

experimental Duſt is raiſed; and yet neither Party can ever be brought to yield; for they are both in the right, only they have been unlucky in giving the ſame Name to different mathematical Conceptions. Had they diſtinguiſhed theſe Meaſures of centripetal Force as *Newton* has done, calling the one *Vis centripetæ Quantitatis acceleratrix*, the other *Quantitas motrix*; all Appearance of Contradiſtion had ceaſed, and their Propoſitions, which ſeem ſo contrary, had exactly tallied.

VI. *A Letter from Rich. Haſſel Eſq; F. R. S. to Peter Daval Eſq; Secr. R. S. concerning a large Piece of a Lath being thruſt into a Man's Eye, who recover'd of it.*

S I R,

Read Nov. 10. 1748. **I** THOUGHT the following Caſe ſo extraordinary as to be worth the Notice of the *Royal Society*. If you think ſo too, I beg you to communicate it.

On Sunday the 17th of Jan. 1747. *Henry Haſſey*, of *South Mims*, Labourer, thruſt a long Lath with great Violence into the great *Canthus* of the left Eye of *Edward Roberts* of the ſame Place Labourer, which broke off quite ſhort; ſo that a Piece two Inches and near a half long, half an Inch wide, and above a quarter of an Inch thick, (*ſee TAB. I.*) remained in his Head, and was ſo deeply buried there, that it could ſcarce
be

be seen, or laid hold of. He rodè with the Piece of Lath in him from *Kick's End*, where the thing was done, to *Barnet*, which is above a Mile, to the House of Mr. *Justinian Morfe*, a Surgeon there, who extracted it with Difficulty; it sticking so hard, that others had been baffled in attempting to extract it. *Roberts* continued dangerously ill a long time; but at last, by the Blessing of God, and the Care of Mr. *Morse*, recover'd intirely, and has the Sight of the Eye, and the Use of the Muscles. But some time after he seem'd well, he told me, that, upon leaning down forward, he felt great Pains in his Head. The Piece is supposed to have pass'd behind the right Eye. I am,

S I R,

Lincoln's-Inn, Nov. 10.
1748.

Your humble Servant,

Richard Haffel.

VII. *The Sun's Eclipse of July 14, 1748. observed at Marlborough House, with the twelve Foot refracting Telescope, fix'd as a Finder to the Tube of the great twelve Foot Reflector; by John Bevis M. D.*

Read Nov.

10. 1748.

Apparent Time.

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July 13. 9. 3. 50 The Beginning, which perhaps
might be 2" or 3" sooner.

Apparent Time.

July 13. 9. 39. 42. The first little Spot in the Western Cluster, quite covered.

52. 00. The biggest of that Cluster quite cover'd, yet somewhat doubtful for flying Clouds.

10. 12. 08. The middle one of three considerable Spots towards the Eastern Limb half cover'd.

The End could not be precisely observed for flying Clouds; at 12. 09. 15. it was not quite over; but at 12. 09. 35. the Sun was clear, and nothing of the Eclipse left.

N. B. *The Wind was so boisterous, that no Phases could be measured with a Micrometer.*

The Moon's Eclipse of July 28. 1748. observ'd at the same Place.

July 28. 10. 13. 28. The *Penumbra* discernible.

06. 30. The Beginning, as most of the Company judged.

18. 38. *Mare Humorum* just touch'd.

26. 24. Began to touch *Tycho*.

27. 51. *Tycho* bisected.

24. 09. *Tycho* cover'd.

29. 53. Touch'd *Grimaldi*.

30. 25. *Mare Humorum* cover'd.

34. 14. *Grimaldi* cover'd.

July

July 28. 12. 24. 30. The End.
27. 40. The *Penumbra* quite gone.

About the Middle of the Eclipse, the Moon's Diameter, perpendicular to the Equator, measur'd in a 5 Foot Telescope was $33' 50''$; perhaps $15''$ or $20''$ greater than it would have been found to be with a 12 Foot Tube.

J. Bevis.

I lately received the following Letter from a Person unknown to me.

S I R, *Luffwick near Thrapston Northamptonshire.*

I HAD the Pleasure to observe the Sun's Eclipse July last, which was as follows. The Beginning $9^h. 1'. 0''$. *a. m.* The End $o. 5. 25. p. m.$ at $10^h. 32''$. $10'. a. m.$ $10^o. 18'$. were dark, which I take to be the greatest with us. These are apparent Times, from a well adjusted Clock (by a Meridian drawn June 10, on a Plate of Metal), and corrected to the Time of Observation.

Our Latitude is $52^o. 27'. 30''$. I hope by this, and future Observations, I shall be able to determine our Situation with respect to yours. I am,

S I R, &c.

Oct. 21. 1748.

Mark Day.

VIII. *An Observation of an extraordinary Lunar Circle, and of two Paraselene's, made at Paris, Oct. 20. 1747. N. S. and of the Eclipse of the Sun, July 14. 1748. O. S. by Augustine Nathaniel Greschow, Memb. of the Royal Acad. of Sciences at Berlin, &c.*

Translated from the French by T. S. M.D.

Read Nov. 10. 1748. **O**CTOBER 20. at Night, the Sky was darkened by a slight Fog, thro' which the Moon appeared of a fiery-red Colour, till 8^h. 40^m. when the Fog was thoroughly dispersed, and the Heavens were overcast with a whitish streaky Cloud. At the same time there appeared round the Moon a Halo (TAB. II. Fig. 7. *ABCD*) accompanied with four other Segments of Circles, two of which *EAF* and *GH* of ten Degrees, were concentric, so as to have their common Center at the Zenith. The Segment or Arch *IPL* on the North Side, of seven Degrees, was concentric with the great lunar Circle, and consequently had the Moon for its Centre; and in fine the Arch *MCN*, which faced the Horizon, was of twelve Degrees.

Besides these four Segments, what was most remarkable was a Mock-Moon or *Paraselene B*, shaped like a Mock-Sun or *Parhelius*. The Diameter of this Mock-Moon, tho' ill-determin'd, was of 35 Minutes at least, with a Tail *BP* opposite to the Moon, as the Tail of a Comet is opposite to the Sun. This Tail varied in its Degree of Light from time to time, extending as far as the Arch *IPL*, which,

which, as well as the Arch *GH*; was 4 Degrees distant from the lunar Circle *ABCD*. The *Paraselenes* *B* had the same Colours with a common *Parhelius*, excepting that they were not so lively, but they very much inclined to the tawny, especially on the Side, which faced the Moon. This *Paraselenes* was in the same Altitude as the Moon. Its Tail was much more faint and transparent; inasmuch as *Capella* appeared thro' this luminous Tail. The lunar Circle *ABCD* was much weaker to the South, and there appeared no *Paraselenes* on that Side. This Meteor did not seem to undergo any Alteration till 9^h. 18'. when the Atmosphere was cover'd with thick Clouds.

The Clouds being diminished at 9^h. 32'. the Meteor appeared again, but very different from what it was before; for, instead of seeing a lunar Circle with 4 other Arches of Circles, I saw the lunar Circle *DABD*, and on the South Side a faint Arch *QR* of four Degrees, having the Moon for its Centre in common with the great lunar Circle. There were likewise two *Paraselenes*'s, one of which *B* was to the North, and the other *D* to the South, as they are expressed in *Fig. 8*. These two *Paraselenes*'s did not cast so strong a Light as that which had appeared before, nor were they so distinctly formed. On the contrary, the lunar Circle was very beautiful, and remarkably bright, until 9^h. 50'. when the whole *Phænomenon* disappeared, and the Sky grew clear by degrees. The Moon's Diameter was 30'. 30''. On the same Night a very beautiful lunar Circle was observed at *Berlin*, but without *Paraselenes*'s.

The following is the Observation that was made of the last Eclipse of the Sun, at the Observatory Royal at Berlin.

1748. July 25. N. S. The Beginning of the Eclipse was not observed, the Sun having been covered with Clouds.

h / "

The *Annulus* was completed at 11 52 51 *antemerid.*

— — — broken 11 54 13

The End of the Eclipse 1 25 9 *post. merid.*

The Diameter of the Sun was 31'. 43''.

This Eclipse was likewise observed annular at *Francfort* upon the *Oder*, but not so exactly as at *Berlin*.

IX. *A Letter from James Parsons M. D. F. R. S. to the President, containing an Account of a preternatural Conjunction of two Female Children.*

S I R,

Read Nov. 17.
1748.

ABOUT the middle of *September* last a Woman in *Holborn* was deliver'd with much Difficulty of two Girls join'd together by the Bellies in so singular a manner, as to deserve a particular Description to be laid before you and the *Society*, as a very curious Subject.

The Care of preparing these Children for keeping in Spirits was committed to Mr. *James Sherwood* Surgeon, who was so kind to send for me to observe them with him; and it was resolved to inject them, in order to make our anatomical Examination the more accurately,

ately, which was very ingeniously executed by Mr. *Sherwood*, and the State of the Children was as follows :

The Skin of Part of the Breast and Belly was continued to each Child, from the lower Part of the *Sternum*, down to the Infertion of a single * *Funis umbilicalis*, which, instead of one to each, serves in common to both.

Each Child had its peculiar Muscles of the *Abdomen* ; but the strait Muscles were so divided, as that the *Rectus* on the right Side of the one Child had the *Linea alba* between it and the *Rectus* on the left Side of the other, and *vice versa* ; so that the Line of each lying directly upon each other, was colligated and open'd, and the Conjunction of the *Musculi recti*, thus formed but one common abdominal Cavity up to the Diaphragms of each Child ; above which each had its own proper *Thorax*, even evident from their external Appearance ; whereas, had their Junction been but never so little in a lateral Way, each would undoubtedly have had its own separate *Abdomen*, since they would not have been so closely pressed forwards, as to occasion that intimate Coalefcion of Parts in the Subject before you ; which is manifest in the Dissections of several of these kinds of Monstrosities, some of which have been join'd by the Hips, some by the Backs, some partly by the Sides, and one or two Cases mentioned by *Parée* and *Tulpius* joined by the Bellies.

None of these uncommon Subjects ought to be touch'd with a Knife, until it is well injected, because the vascular System, where there are any preternatural Adhesions

* See a similar Case in these *Transf.* No. 65, p. 296.

Adhesions or Distortions, can never be understood nor traced without it; and therefore *Tulpius*, whose Account of his Subjects is very inaccurate, and who certainly did not inject it, confesses he could make no Distribution of the Vessels, nor find out any thing of them distinctly.

But in the present Case, a complete Injection of the Children being made by the Vessels of the umbilical Cord, we were enabled to give the following exact Account of the vascular System and other Parts; to which however we shall premise a Description of the intestinal Canal of both.

When we came to examine the Intestines, the only proper means for laying them fairly to View, before they were taken out of the Body, was to inflate them; which was accordingly done, and thereby every Part of them was rendered as conspicuous as the Drawing now before you, and of the same Size exactly. Each Child had its own peculiar *Oesophagus*, *Stomach*, and *Pylorus*, in a natural State; from each of which the *Duodenum* descended about three Inches, and then united into one common Duct, which we shall call the Beginning of the *Jejunum*, and which was near four Inches long: This was inserted into the upper Part of a large *Sacculus*, formed out of the very Coats of the Intestines, and differing in no wise from them in Colour, Density, or any other Quality but the Form and Extension.

Its horizontal Diameter was about 5 Inches, and its vertical about 4, and it was formed out of the *Jejunum*, which, in some Subjects, is as long as the *Ileum*, in most near that Length, and no doubt
was

was an Attempt of Nature to supply the Want of two regular *Fejuna* : For we are to observe, that if these Children had lived, each having its own proper Stomach, would probably have eaten a due Quantity of Food for its Sustenance; and the Office of the Stomachs might have been well enough performed; but each requiring a separate System of Intestines to dispose naturally of the digested Chyle, and this preternatural Conjunction happening between them, the *Fejuna* of both were confused together; and having Room in the *Abdomen*, now large and common to both, these Parts of their Organizations, that ought to have grown into two Guts of a considerable Length, being hindered from a regular Accretion, the joint growing Powers of both formed the Sack of Communication now before you; which is proportionably capacious enough to answer the Purposes of two natural *Fejuna*; below which the rest of the Intestines of each Child were sufficient to do their several Offices.

In the lower Part of this *Sacculus* there was an Outlet on each Side, which were the Origins of their separate *Ilea* : These were in a good State, and regularly inserted each into its *Cæcum*; and this in each had its natural *Appendicula*; these were regularly succeeded by their *Colons*, and terminated by their proper *Recta intestina* to their natural Outlets; with this Difference only, that the *Colons* were out of their natural Situation, and were convoluted in each Child, by as narrow Portions of the *Mesocolon*, as any Part of the *Ileum* is by its Mesentery; and that as low as the going off of the *Rectum*.

Of

Of the Vascular System.

We are to take notice, that as these Children had but one abdominal Cavity between them, so it contained, in Appearance, only one Liver of a considerable Size, and an irregular Form; but this consisted of two in Reality preternaturally joined, as there were two Gall-Bladders. The umbilical Vein is inserted into this, pretty nearly in the usual manner, and afterwards this Canal is divided into two Branches, which carry the Blood into the *Vena cava* of each Child; whence it falls naturally into the right Auricles of their Hearts.

The Heart of the larger Child is but small, has a bifid *Apex*, and from the Division has a Vestige of the *Septum*, on both the upper and under Sides; which forms a *Sulcus* in a longitudinal Direction, from between the *Apices* to the *Basis*; from whence arises a *Pericardium* which extends itself over each Side from the *Sulcus*, and so forms a separate *Cap-sula* over each Ventricle of this Heart, and may therefore be call'd a double *Pericardium*.

The ascending Vessels are distributed according to the Standard of Nature; but the descending Trunk of the *Vena cava* rides over that of the *Aorta*, above the going off of the Emulgents, and sinks back again behind the external *Iliac Artery*, before it is itself divided into *Iliac Veins*, descending naturally to the lower Extremities, as do the Arteries from thence also.

The Kidneys, urinary and uterine Parts were in a natural State; and the Lungs appeared well, and seem'd as if this Child had breathed.

The

The Heart of the smaller Child was single, but above a third larger than it naturally ought to be ; out of which the ascending Arteries are very regular and natural ; but there was scarce any Vestige of Lungs in this Child on the right Side of the *Thorax*, and but a small Portion of the pulmonary Substance in the left. The descending Trunk of the *Aorta* is very small in comparison of the other ; yet goes down regularly towards the Extremities, except the internal *Iliac Arteries*, which were obliterated and degenerated into Ligaments, whilst the Externals continued down, as I have just said ; for only the *Iliacs* of the larger *Fœtus* took place in the umbilical Cord, which was the Reason that we found but two Arteries in it ; so that, altho' both Children received Nourishment by the Division of the Canal from the Liver to the *Vena cava*, yet the superfluous Blood of both could be sent back to the *Placenta* no other way than by the internal *Iliacs* of the greater Child.

The descending Branches of the *Vena cava* enter'd as usual, on the right Side, into the Auricles ; but those of the left join in one Trunk, pass round the left Auricle, and enter into the right close by the *Cava ascendens*, which is of a natural Size, and very regular up to the Diaphragm, from which it extends a full Inch before it reaches the Auricle ; the Kidneys differ a little in Size from each other ; yet these, with the other urinary and also the uterine Parts in general, are in good Order ; but the most remarkable *Lusus* of Nature in these Subjects is an Artery which arises from the *Aorta* about the Place of the *Celiac* of the one Child, running along

before the Liver, and is inserted into the same Place of the *Aorta* of the other. It was much larger than any other Artery in either Child, and bestow'd Branches on the *Stomach*, *Mesentery*, and *Mesocolon*; being about five Inches long; for there were neither *celiac* nor *mesenteric* Arteries, according to Nature in either Child.

Observations.

When some former Authors (before the Learned were so happy as to know the Sweets of experimental Philosophy) endeavoured to account for monstrous Productions in the animal World, they could have recourse to no other means to explain them, than to the then reigning Systems by which they usually explained the *Phænomena* of the natural Generation of Animals; and that was in general, that an Animal was produced by the Admixture of the supposed seminal Matter of both the Parents; that the Quantity produced by this Commixtion was supposed always to contain only a Quantity of Particles sufficient to produce a natural *Fœtus*, by the Mediation of a certain *plastic* Power, said always to attend it, as well as any other natural Production in the World.

Upon this Plan many little Alterations were made by succeeding Authors, without differing widely from this general Notion; all as liable to Objections, and as easily refuted as the Source from which they sprung. And notwithstanding the Truths that have since been traced out by later philosophical Advances, leading to a more reasonable Knowledge of the Subject, yet there still are some who
appear

appear unwilling to quit these old Errors. But as this is not the Place intended for a Discussion of the several Opinions concerning it, we shall here only consider how the Conjunction of these two Children happen'd; as well as the means whereby Children acquire superfluous or want the necessary Members, or are any otherwise deformed.

According to a late Reading I had the Honour to exhibit before you upon the Analogy between the Propagation of Animals and Vegetables*, I hope it appear'd pretty clearly, that both these Parts of the Creation are daily propagated from Organizations already formed and treasured up in natural Receptacles provided for them, till they come to be removed into proper Places of Nourishment; from which Opinion we can find no Reason to swerve at present, and which we must have recourse to, in order to account for the present Subject.

If the old Systems had Weight formerly, later Enquiries have exposed their Absurdities. Truth admits of nothing absurd, and as to what regards the Works of the Creation, especially that of Animals and Vegetables, that System of accounting for their Generation which is most simple, and is least liable to Objections, is most likely to succeed in the Enquiry; and since the Ordination of Providence was that all should be good, it will appear the means of the Propagation of Animals and Vegetables, which was partly the Subject of the Discourse lately read before you, will seem least of all liable to any Accidents that might degrade the ge-

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neral

* Which I design shortly to publish. J. P.

neral Usage and Standard of Nature, by the Production of Monstrosities in any Part of her Works.

When the *Vis plastica* was thought to be the Agent for the Guidance of the Work of Generation, and that a monstrous Child appear'd, it was blamed by the Authors of most Nations in some of these Particulars; the seminal Matter was either in too small or too great a Quantity, and the *Vis plastica* puzzled in the Management of the Particles that go to form the different Parts of the Animal; or it was sometimes careless and negligent in the Application of the Ideas of some or other of the Parts, and consequently the Animal must want some Limb or other; or, from a Superabundance of Matter, have superfluous Limbs added to them by this unskilful erring Agent: But in whatsoever manner the Commixtion of the male *Semen*, with that ignorantly supposed to be in Females, and the Formation of a *Fœtus* therefrom, is said to be conducted, the Accidents and Chances against the Welfare of all animal Beings would be so numerous, and the State of Nature so miserable, that the greatest Part of the Inhabitants of the Earth and Waters could not avoid being monstrous, and full of Confusion: The Almighty would have produced an Effect contrary to His Divine Goodness, and Care for His Creatures; and, in fine, it would be highly absurd, to suppose the Regulation of things of this high Consequence to be committed to any finite subordinate ignorant Agent, which must undeniably be insufficient for this great Work.

But the System of Generation which supposes the Organizations of Animals and Vegetables already formed with an Incapacity of growing into any
other

other Forms than those of their Parents, is the most secure from any Confusion, or any preternatural Digressions from their due Forms, is most compatible with that gracious Design, that all should be good: For every animal and vegetable Body is daily seen to be constant to its own Kind, and can be subject to no Accident but one to render it monstrous in its Accretion, and that in general is Compression: For all animal and vegetable *Ova* are most certainly perfect in their first Formation, as the Seeds of the latter plainly shew, and in a State of Rest, until they are deposited in their natural *Matrix*, be it sooner or later; the *Ova* of Females in the *Ovaria* during Life, and the Seeds of Vegetables in our Repositories for any Number of Years; and, after that, would certainly, without Accidents or Interruption, continue their perfect Form to their utmost Growth.

From hence it will be easy to account for the preternatural Adhesion of these two Children, and the Confusion of their *Viscera*, upon the most easy Plan, and most simple and persuasive Reasoning imaginable; and from hence also we shall be able to account for every other Monstrosity that can attend Animals and Vegetables,

We have observed before, that each Seed and *Ovum* contains the Animal and Vegetable proper to its Species. Now, when two or more of these animal *Ova* are fecundated, and come into the *Uterus*, the Sides of the *Ova* (which are the Membranes that contain the Fluids in which the little Organizations swim) must inevitably come into Contact; and if the Membranes of each continue in a good State, the *Fetus's* will be free in their
several

several Apartments, and grow proportionably; but if the Parts of the Membranes, which are close together, by being thin and weak, or by any irregular Resistance, or Friction, come to be dissolved or broken, then the Fluids of both unite, and the two little Organizations, having no longer a Partition between them, come together, adhere, and intwine into each other, their tender Parts easily coalescing; and from the natural Disposition of each to grow and increase, their Accretion goes on, there is a mutual Insinuation of Vessels where the Parts are compressed, and a mutual confused Circulation carried on, and at length the Whole becomes irregular and monstrous.

We have many Facts to corroborate this Opinion, and to shew that the Fibres of Animals and Vegetables have a wonderful Capacity of extending and insinuating themselves into one another; and of continuing a Circulation reciprocally; and the Blood-Vessels of being elongated, and even of producing new Ramifications where the Restoration of a Part requires it, from the Principles I before laid down in the first Part of my Analogy; else how should wenny Tumours of a monstrous Size be propagated on the Surfaces of human Bodies? how should the Lungs adhere to the *Pleura* so intimately as to become one entirely united Mass, as incapable of being separated without a Knife as any Part of a Muscle? How should the *Sarculi* or *Buds* of Trees implanted into others by Grafting or Inoculation, so insinuate their Fibres into those of the Stock in which they grow, as to become

become one continued Piece of Wood with them? How are the *Sutures* of the *Scull*, and those of the *Epiphyfes* of Bones, totally obliterated in an advanced Age, but by the Insinuation and reciprocal Combination of the Fibres of each other? And, in a word, how are many recent deep Wounds so soon agglutinated, if there be not a speedy Insinuation of Vessels, and a Circulation soon carried on?

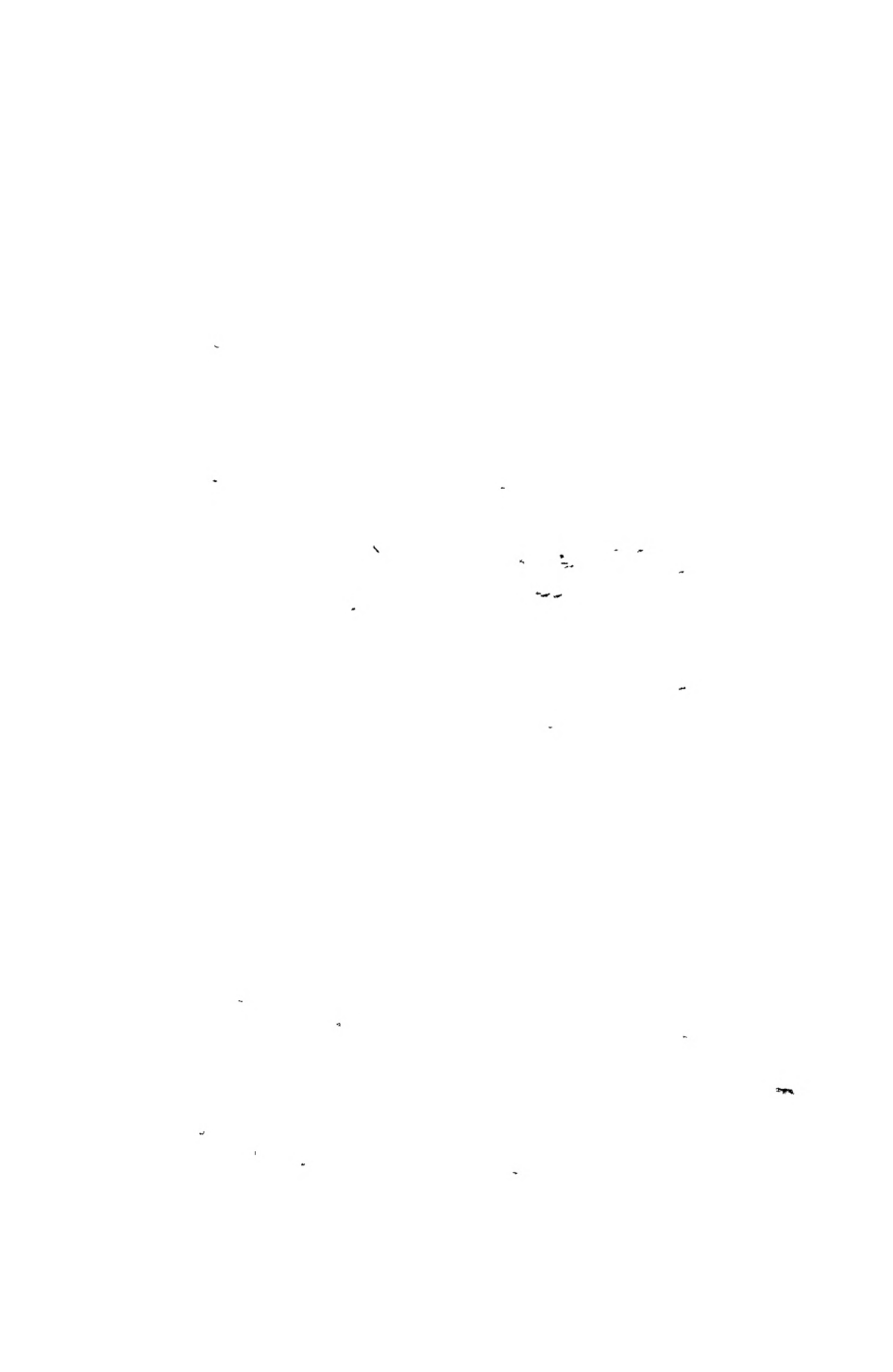
Indeed if we are only to look on, and consider the Subject before us in its present State, it will be somewhat difficult to conceive how this strange Conjunction could happen; but we are to go back, and consider two minute tender Organizations, whose remotest Parts from each other might not exceed perhaps the fiftieth Part of an Inch at the time of their Adhesion, and the Difficulty is taken away. Thus our Children happening to be compressed by their Bellies, the tender Integuments between the *Musculi recti* in each were soon thinned and dissolved, the Coalescion happen'd as I have said before, and the Intrusion and Commixtion of Parts, that appear before you, was begun and carried on by their growing wheresoever the Resistance was least.

And thus if the Contact and Pressure of the two *Fetus's* be pretty equal and moderate, they will grow equally; if the Compression be very great, and both be compressed all round, having no Room, because of the *Uterus* not giving Way, the Confusion of both, or indeed of one only, will be so great, as to cause a Mass without any Form or Regularity at all. If their Contact be so disposed, as that one *Fetus* is much compressed and confined, the other
has

has sufficient Room, this will grow proportionably, whilst the Growth of the other being intirely stopped from the Beginning in its minute State, except perhaps an Arm, or Leg, or Head, &c. that which has Liberty will have such superfluous Limb or Part growing with it, as remained uncompressed, whilst the rest is obliterated and lost.

The same is also apparent in Vegetables: A Carrot, Parsnip, Radish, and such-like Vegetables as naturally grow strait and well-form'd, may be distorted and alter'd at Pleasure, as they grow, by Compression: For, as the nutritious Juices are equally distributed, and attracted in the same Quantity for the Use of the Whole; if a Compression be made on any Part, those Juices, which are hinder'd to flow into the compressed Part, will be determined elsewhere, and form Gibbosities and Deformities in other Parts of the Organization, where the Resistance is less, and the Whole become changed from its natural Form. Thus *Gourds*, as they grow, in applying Pressure by Ligatures, or otherwise, may be brought to various Forms; and *Apples*, placed in cylindrical *Phials*, whilst small, will, by the lateral Pressure, lose their roundish Form, and acquire that of a *Cylinder*. And thus Nuts, Apples, &c. may be conjoin'd and become double; and not because there was a Superfluity of a supposed Matter to form them, by any subordinate Help: And thus also a Nut or Apple, &c. among a Bunch of sound ones, may, by Compression of its Organization, be vitiated and ill-form'd; and not because there wanted a Sufficiency of this supposed Matter.

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TAB. III.

Fig. 1.
p. 539.



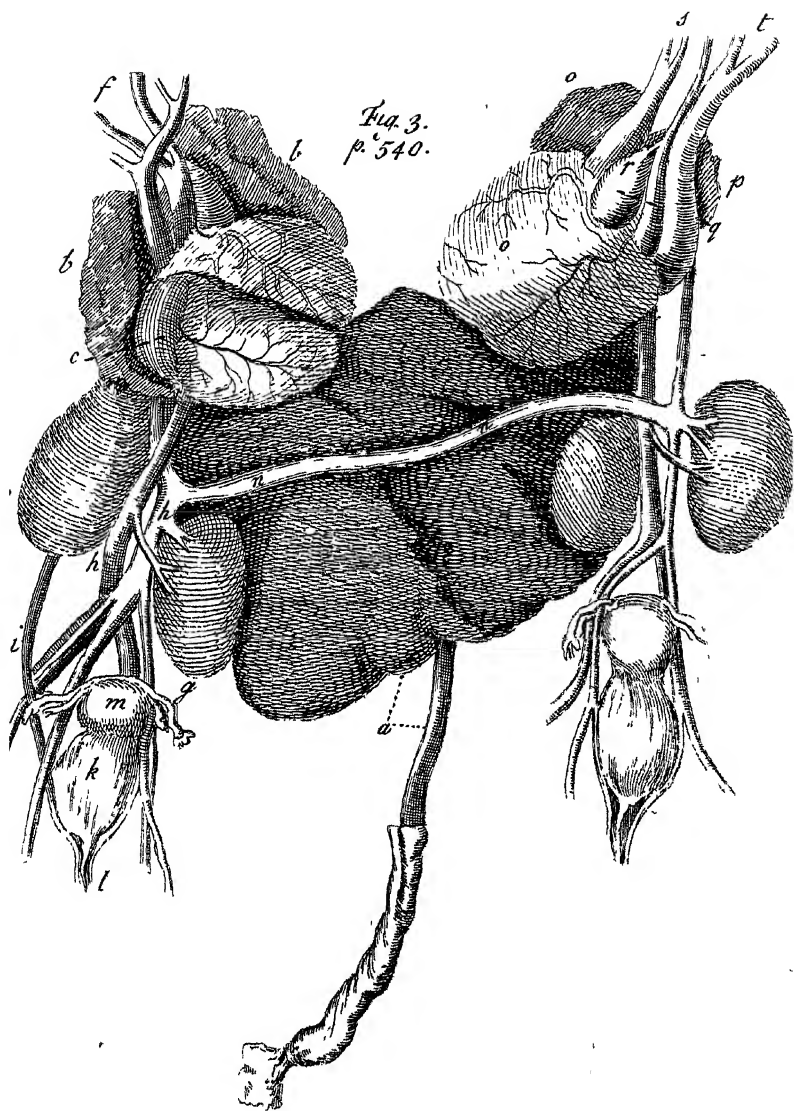


Fig. 3.
p. 540.

J. Mynde sc.



In this manner, it is highly probable, all animal Monstrosities happen; and this was the Case of the *Cow*, which many of us saw a Fortnight ago. Her twin Sister happen'd to be confin'd and compressed to her Spine; nothing remaining free but the *Abdomen*, the Dugs, one of the anterior Extremities, and the Dew-lap; every other Part was obliterated, whilst these continued to grow, by the Communication of Vessels between them, in the manner above explained.

This, Sir, is the Sum of what I am capable of conceiving concerning monstrous Productions: I shall think myself happy in the Concurrence of this learned *Society* with my Opinion; and am, with the truest Respect,

S I R,

Your most obedient Servant,

James Parsons.

An Explanation of the Drawings representing the above-described Two Children joined together.

TAB. III. *Fig. 1.*

Represents two Female Children preternaturally join'd by the abdominal Integuments, from the *Umbilicus* up to the *Cartilago ensiformis*, in such a manner, as to form between them but one *Abdomen*.

B b b b

TAB. IV.

TAB. IV. *Fig. 2.*

Shews a back View of the intestinal Canal of each Child, from the Stomach to the *Anus*.

a, a, The Stomach of each.

b, b, The *Duodenum*.

c, c, Part of the *Jejunum*, which is common to both Children.

d, The remaining Part of the *Jejunum* form'd into a *Sacculus*, out of which,

e, e, The *Ileum* of each Child arises.

f, f, The *Cæcum* of each.

g, g, The *Colon* of each.

h, h, The *Rectum* of each.

TAB. III. *Fig. 3.*

Is a fore View of the other *Viscera*, and vascular System of both Children.

a, the umbilical Vein entering into the Liver, which is form'd of that of each Child preternaturally conjoin'd.

b, b, The Lungs of the larger Child.

c, The Heart, which has a bifid *Apex*, and of which each Ventricle has its particular *Pericardium*, from the *Sulcus* that divided it.

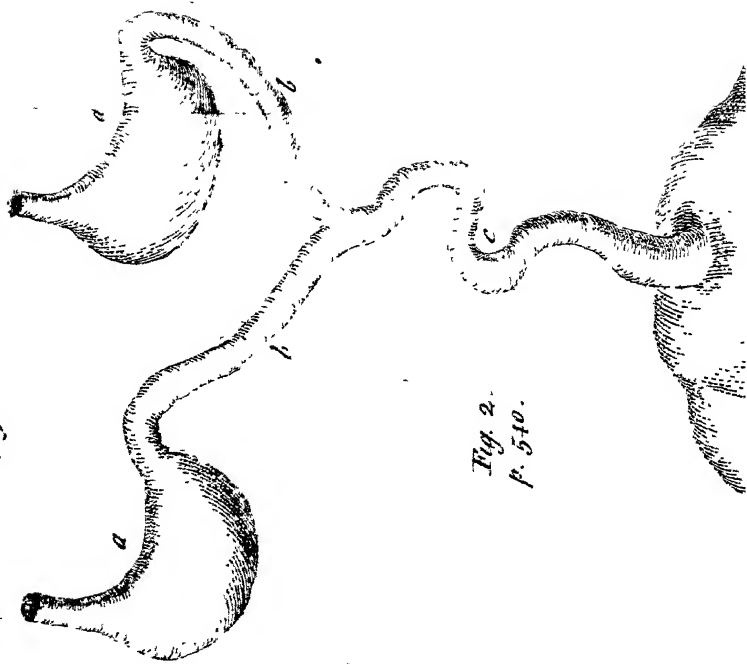
f, The great Vessels arising out of the Heart.

g, g, The Kidneys in some measure misshapen.

h, h, The Trunks of the large Vessels descending to the lower Extremities. The Vein running before the Artery, and sinking behind it again where it divides.

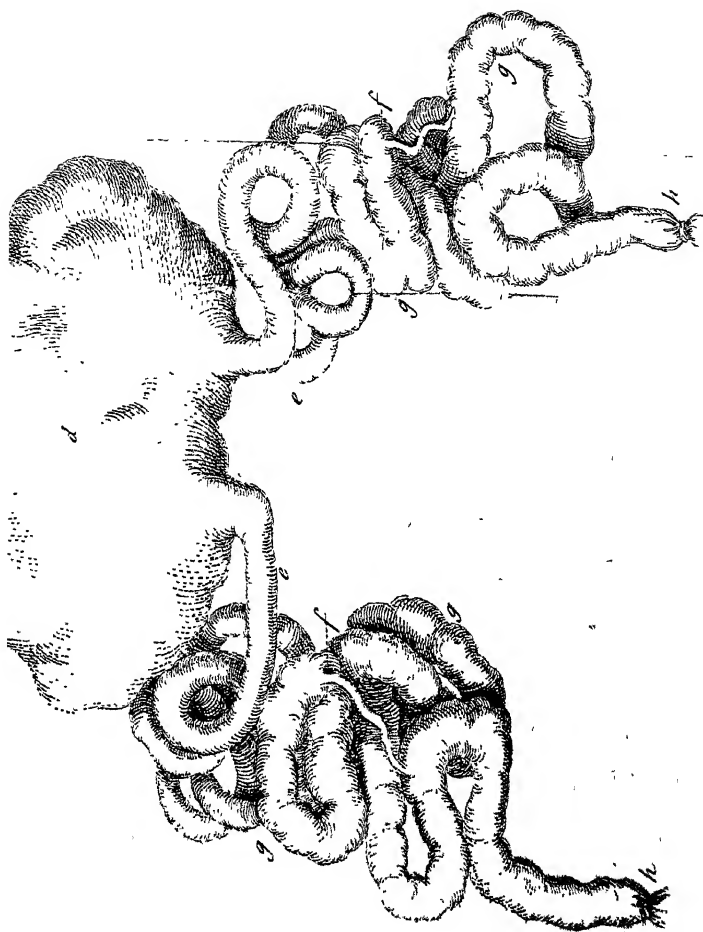
i, The right *Ureter*.

k,



*Fig. 2.
p. 540.*

J. Myndes, Jr.





- k*, The Bladder ; and
l, The umbilical Arteries, with the *Urachus* turn'd down, to shew
m, The *Uterus*, &c.
n, *n*, Is an Artery communicating with, and entering into, the *Aorta* of each, near the going off of the Emulgents.
o, The Heart of the smaller Child, much too large in proportion, together with the right Auricle.
p, Part of the Lungs, which were render'd much too small, in proportion, by the Compression of the large Heart upon them.
q, The *Aorta* and pulmonary Artery, as they are connected by the *Canalis Arteriosus* (*r*).
s, *t*, The descending Trunks of the Veins; the latter of which was preternatural, running round the left, and entering into the right Auricle in its posterior Part.

The other Parts were much as those of the former Child in general ; except the *Aorta*, which was much smaller, as the Figure shews.

XI. *An Account of the Preparation and Uses of the various Kinds of Pot-ash ; by John Mitchell M. D. & F. R. S.*

Read Nov. 17 and 24-
 1748.

ALTHO' Pot-ash is a thing daily used, and well known even to the Vulgar ; yet, as the making it is a mechanic Art, practis'd only by the Vulgar, and neglected and overlooked by the Learned, so we have had

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no

no satisfactory Account of it; and they, who understand it, generally keep it a Secret, lest others should learn so beneficial an Art. But as it is a Commodity that no Nation hardly can well be without, either for making Soap, Glass, Dying, or Bleaching, so the Way of making it is generally understood in most Countries, except our own. For in *France*, and other Countries where they make Wine, they make a kind of Pot-ash in an easy manner from the Lees of their Wine. In those and other more Southern Climes, they have many kinds of Herbs hereafter mention'd, either spontaneous, or cultivated on purpose, which they as easily convert into Pot-ash. In *Germany*, and other more Northern Countries, they make great Quantities of Pot-ash by extracting the Salts of their Wood-ashes, in a manner that is well known. But it is only in *Russia*, *Sweden*, and other Northern Nations, where the Art of converting their Wood-ashes into Pot-ash, without the tedious Process of Elixivation, is either well known to the Learned, or practised by the Vulgar.

By this means most Nations are supplied with this necessary Commodity of their own, except the *English*, who might be supplied with any Quantities of it, from the great Plenty of otherwise useless Wood they have in their Colonies, if not at home, if they knew how to make it. But it seems this Art is so little understood among us, that many Attempts I have known to make Pot-ash have all proved unsuccessful merely upon that account, so as to be intirely laid aside. This has put me for some time upon inquiring into the Ways of making
this.

this Commodity, of which several have been suggested to me, from the several Trials and Informations hereafter mentioned.

It is well known, that the Ashes of all kinds of Vegetables whatever afford Pot-ash in some measure or other; altho' some are much more fit for that Purpose than others, which may be determined from the Experiments of *Redi* in the *Philosoph. Trans.* N°. 243, p. 281. *Boerhaave*, *Merret*, and others; so that we need not insist upon them here.

As for the Trees and Herbs of our Colonies in *North America*, most of those that are common in their Woods are known to be fit for this Purpose, as the Ashes of them all, burnt promiscuously in their Houses, make a very strong Lye fit for Soap. Of these, the fittest for that Purpose is their *Hicory*, the most common Tree in their Woods, which makes the purest and whitest Ashes, of the sharpest Taste, and strongest Lye, of any Wood I have seen. Their *Stickweed* is said to do the same; which is as common a Weed. For this Reason the Ashes of both these Plants were used by our *Indians* there, instead of Salt, before they learnt the Use of common Salt from the *Europeans*. The Ashes of *Tobacco* likewise, when damnified, or not fit for a Market, or its Stalks, Stems, and Suckers, of which great Quantities are thrown away, and rot and perish, are very fit for Pot-ash, as they contain a great deal of Salts, and are well known to make a strong Lye.

On the other hand, *Pines*, *Firs*, *Sassafras*, *Liquid Amber*, or *Sweet Gum*, or all odoriferous Woods, and those that abound with a Resin or Gum,

Gum, are unfit for Pot-ash, as their Ashes are well known, even to our Planters, to make a very weak Lye, unfit for Soap.

Besides these that contain little or no Salt, there are some other Vegetables that afford a large Quantity of it, but make a bad kind of Pot-ash, at least for many Purposes, on account of a neutral Salt with which they abound. This seems to have been the Case of the Pot-ash made in *Africa*, in a Manufacture of that Commodity set up there by the *African* Company, which Mr. *Houston* (who was chiefly concerned about it) tells us, in his *Travels*, proved so bad, on account of a neutral Salt it contained, that the Manufacture was left off on that account; or, perhaps, from their not knowing how to make it aright. What those Vegetables are, that afford this kind of Ash, is not well known, if it be not Fern, and some Sea-Plants.

Whatever Vegetables we make our Pot-ash of should be fresh or green, and no-ways rotten, dried, or decay'd. They should likewise be burnt to Ashes by a slow Fire, or in a close Place; otherwise, when they are burnt in the open Air by a strong Fire, great Quantity of the Ashes is consumed in Smoke, by the saline and terrestrial Parts being carried up in Fumes, before they are separated from these exhalable Parts by the Action of the Fire. For the Difference between burning Wood in a close Place, or the open Air, is so great, that the Quantity of Ashes obtained from one is more than double the other. This we learn from the Experiments of *Lundmarck* hereafter mention'd, who tells us, he burnt a Quantity of Birch in a close

close Stove, from which he obtained five Pounds of Ashes; whereas the same Quantity of the same Wood burnt in the open Air, yielded only two Pounds.

It is for this Reason, that most People who make Pot-ash, burn their Wood in Kilns, or Pits dug in the Ground; altho' the *Swedes* burn it in the open Air, as the Author above-mention'd informs us. This first Step, or the burning the Wood to Ashes, seems to be taken by many for the whole Process of making Pot-ash; for they who pretend to have learned this Art in *Russia*, as well as *Lemery* and some other Authors, hardly give us any other Account of it.

But, in order to convert the Ashes, prepared in this or any other manner, to what is called Pot-ash, there are many different Ways practised in different Countries, which make as many different kinds of Pot-ash, that are all to be found in our Markets, and have all their respective Uses.

1. The first of these is commonly called Pearl-ashes by our People, who import great Quantities of it from *Germany*. This is no other than the lixivial Salt of Wood-ashes, extracted by making a strong Lye of them, and by evaporating it to Dryness, in a manner that is well known, and sufficiently explained by *Kunkelius* in his Art of making Glass, *Boerhaave*, and many others; so that we need not insist upon it here; we shall take a more fit Opportunity to explain it, for the Use of our People in *America*.

2. But the Art of converting these Wood-ashes into Pot-ash, without this tedious Process of Elixivation,

is only practised in *Russia, Sweden*, and other Northern Countries, where it has been lately disclosed by one *Lundmarck*, who tells us he had often made it himself, in the manner he now describes. This Account is contained in an academical Dissertation upon this Subject at *Aboe* in *Sweden*, and was communicated to me by Dr. *Linneus*, Professor of Botany at *Upsal*, as a genuine Account of this Art; which I think has hitherto been generally unknown.

This Author tells us, “ They have many large
 “ Woods of Beech in *Smoland*, and other Parts of
 “ *Sweden*, in want of which they take Alder: Of
 “ these they are allowed to use only the old and
 “ decaying Trees for this Purpose, which they cut to
 “ Pieces, and pile in a Heap, to burn them to Ashes,
 “ upon the Ground, by a slow Fire. They carefully
 “ separate these Ashes from the Dirt or Coals in them,
 “ which they call raking them; after which they col-
 “ lect them in Baskets of Bark, to carry them to a Hut
 “ built in the Woods for this Purpose. This they con-
 “ tinue to do till they have a sufficient Quantity of
 “ these Ashes. Then their whole Art follows; for
 “ which they choose a convenient Place, and make
 “ a Paste of these Ashes with Water, by a little at
 “ a time, in the same manner, and with the same
 “ Instruments, as Mortar is commonly made of
 “ Clay or Lime. When this is done, they lay a
 “ Row of green Pine or Fir-Logs on the Ground,
 “ which they plaster over with this Paste of Ashes:
 “ Over this they lay another Layer of the same sort
 “ Logs of Wood, transversely or across the others,
 “ which they plaster over with the Ashes in the same
 “ manner.

“ manner: Thus they continue to erect a Pile of
 “ these Logs of Wood, by Layer upon Layer, and
 “ plastering each with their Paste of Ashes, till they
 “ are all expended; when their Pile is often as
 “ high as a House. This Pile they set on fire with
 “ dry Wood, and burn it as vehemently as they
 “ can; increasing the Fire from time to time, till
 “ the Ashes begin to be red-hot, and run in the
 “ Fire. Then they overset their Pile with Poles,
 “ as quickly as they can; and while the Ashes are
 “ still hot and melting, they beat and clap them,
 “ with large round flexible Sticks made on pur-
 “ pose, so as to incrust the Logs of Wood with the
 “ Ashes; by which the Ashes concrete into a solid
 “ Mass as hard as Stone, providing the Operation has
 “ been rightly performed. This Operation they
 “ call *Walla*, i. e. *Dressing*. At last they scrape
 “ off the Salt thus prepared, with iron Instruments,
 “ and sell it for Pot-ash; which is of a bluish dark
 “ Colour, not unlike the *Scoria* of Iron, with a
 “ pure greenish white Salt appearing here and there
 “ in it.”

All the Pot-ash we have from *Russia*, *Sweden*,
 and *Dantzick*, is exactly like what our Author here
 describes, and seems to be made in this manner. It
 is, however, generally observed, that the *Russian* is
 the best of these, on account of the greater Quan-
 tity of Salt in it. Now if, in the preceding Process,
 we make our Paste of the Ashes with Lye, instead
 of Water, it is plain the Pot-ash will be impregnated
 with more Salt, and make all the Difference there
 is between these Sorts of Pot-ash. This then is
 likely to be the Practice in *Russia*; where their

C c c c

Wood

Wood may likewise be better for this Purpose, and afford more Salt. This is well known to be the Case of different Kinds of Wood: So our Author above-mentioned tells us, he obtained $2\frac{3}{4}$ lb of Salt out of eight cubic Ells of Poplar, which was very sharp and caustic; but the same Quantity of *Birch* afforded only one Pound of Salt, and that not so strong; and *Fir* hardly yielded any at all.

The Way of making Pot-ash above-described may be the more easily understood by our People in *America*, for whom this is chiefly intended, as it is the same with their Way of making Lime of Shells, the only Lime they use in most Places. These Shells they burn to Lime between the Layers of a Pile of Wood (instead of a Kiln) till it is reduced to Ashes, in the same manner as is here directed to be done with Ashes, to make Pot-ash. The Lime, thus made, is reckoned very good; but, as it is impregnated with the Ashes of the Wood, and the marine Salt that is often in the Shells, it is apt to make the Houses that are built with it very damp in moist Weather, so that the Water often runs down their Walls in Streams; which cannot but be very unwholesome in an Air that is naturally close and damp: The only Way to prevent which would be, to wash and dry their Shells frequently, and burn them in dry Pine, that afford little or no lixivial Salt. But to return to our Purpose:

3. There is another Way of making Pot-ash, practised chiefly in *England*; where they make it in the following manner, as I am informed by several, who have seen it done:

With

With their Ashes of Fern, or Wood of any kind, they make a Lye, which they reduce to what they call Pot-ash, by burning it with Straw. To do this, they place a Tub full of this Lye nigh a clean Hearth of a Chimney, in which they dip a Handful of loose Straw, so as to take up a Quantity of Lye with it. The Straw thus impregnated with Lye they carry as quick as they can, to hold it over a blazing Fire on their Hearth, which consumes their Straw to Ashes, and at the same time evaporates the Water from the Salts of the Lye. Over the Blaze of the first Parcel of Straw they burn another dipt in Lye in the same manner. This they continue to do till their Lye is all expended. By this means the Coals and Ashes of the Straw, and Salts of the Lye, are left on the Hearth, and concrete together into a hard solid Cake of a greyish black Colour, which they scrape off, and sell for Pot-ash.

This is an easy Way of making Pot-ash, in want of proper Vessels to extract the Salt of the Lye by Evaporation, or in want of Wood to reduce the Ashes to Pot-ash in the Way above-mentioned, for which it seems to be contrived, and for which it is only to be commended. For the Pot-ash made in this manner is full of the Coal of the Straw, and its Salt is not so strong, as our Workmen say, or so sharp and corrosive as the Salt of the foreign Pot-ash, that is calcined in an open Fire; besides other Differences hereafter mentioned; which makes this Pot-ash unfit for some Purposes, and not above half the Value of the foreign.

4. They have a very different Way in the North of *England* of reducing their *Kelp* to Pot-ash, which they use for making Alum. This is made of the different Kinds of *Fuci*, or Sea-Weeds thrown up on the Shore, or gather'd on the Rocks; which they dry a little in the Sun, and afterwards burn them in a Kiln, built of the Stones they find on the Shore, in a cylindrical Form, and about two Foot or less in Diameter. In this they first burn a small Parcel of the Herb, and before it is reduced to Ashes they throw on more, till the Kiln is full, or their Materials are expended. This is said to reduce the Ashes to a hard and solid Cake, by the Heat of the Kiln, and Quantity of Salt in the Herb, which makes what is commonly called *Kelp-Ashes*.

There are some other Ways of making Pot-ash, suggested by several, both Authors and others, which appear to be more easy and ready than any of the above-mentioned; for which Reason they are apt to be tried by those who make Attempts of this kind. These are deduced from what they reckon the Nature and Properties of this Production: And there is no doubt, but if that was well understood, it might afford some Insight in the Way of making it. For this Reason we made the following Experiments with the best *Russia* Pot-ash, in order to discover its Nature and Properties, and how they are most probably communicated to it; that we might see what we are to make; in order to imitate the best, or to make what is accounted good Pot-ash.

1. *Russia* Pot-ash, as it is brought to us, is in large Lumps, as hard as a Stone, and black as a Coal.

Coal, incruſted over with a white Salt, that appears in ſeparate Spots here and there in it.

2. It has a ſtrong fetid ſulphureous Smell and Taſte, as well as a bitter and lixivial Taſte, which is rather more pungent than other common lixivial Salts.

3. A *Lixivium* of it is of a dark-green Colour, with a very fetid ſulphureous Smell, and bitter ſulphureous Taſte, ſomewhat like Gunpowder, as well as ſharp and pungent like a ſimple *Lixivium*.

4. Altho' it is as hard as a Stone, when kept in a cloſe Place, or in large Quantities together in a Hogſhead; yet, when laid in the open Air, it turns ſoft; and ſome Pieces of it run *per deliquium*; whiſt moſt other kinds of Pot-aſh only turn friable, and crumble in the open Air.

5. It readily diſſolves in warm Water, but leaves a large Sediment of a blackiſh grey Colour like Aſhes, which is in a fine ſoft Powder, without any Dirt or Coals in it; that are to be obſerved in moſt other kinds of Pot-aſh.

6. As it is diſſolving in Water, I have ſcummed off from ſome Lumps of it a dark-purple bituminous Subſtance, like *Petroleum* or Tar, which readily diſſolved in the *Lixivium*.

7. This, or any other true Pot-aſh, or a *Lixivium* made of them, will preſently tinge Silver of a dark-purple Colour, difficult to rub off; whiſt a mere lixivial Salt has no ſuch Effect.

8. Pieces of this Pot-aſh boiling in Water make a conſtant Exploſion like Gunpowder; which was ſo ſtrong as not only to throw the Water to ſome Height, but to liſt up and almoſt overſet a ſtone Cup
in

in which I boiled them. These Explosions were owing not so much to the included Air, which some perhaps may imagine, as to the sulphureous Parts of the Composition expanding and flying off: For this boiled *Lixivium* had neither the green Colour, nor fetid sulphureous Smell and Taste; at least in any degree like what it has when made of the same Pot-ash by a simple Infusion in warm Water.

9. I evaporated some of the green *Lixivium*, made only by Infusion, and filtered thro' a double Rag: As soon as it began to boil, a green Powder, to which its Colour is owing, fell to the Bottom, and the Lye became pale. After it was evaporated to a Pellicle, and set in a cool Place, a Salt separated from it on the Sides of the Cup, in angular Crystals like Tartar. These Crystals were soon formed, and in pretty large Quantities, but were difficult to separate from the alkaline Lye and Salt, in which and the open Air they were apt to dissolve: But from the Pellicle I obtained some Pieces of the same Salt that would not dissolve in the open Air.

10. Oil of Vitriol makes a strong Effervescence with this green Precipitate, with a white Fume, and a very strong sulphureous Smell. It does the same with these white Crystals, altho' the sulphureous Smell is not so strong. But with the pure fixed Alkali there was no such sulphureous Smell to be discerned.

From these Experiments we may determine something about the Nature and Contents of Pot-ash. This we are the better enabled to do, from the
accurate

accurate Experiments and Reasonings of the learned Mr. *Geoffroy*, on a like Substance made of Charcoal and an Alkali Salt calcined together, in which he observed all the Properties and Contents of Pot-ash above mentioned, particularly related in the *Memoirs* of the *Royal Academy*, for the Year 1717. This was made of the same Materials, and had all the Properties above-related of our Pot-ash; particularly a green *Lixivium*, a strong sulphureous Smell and Taste, a sulphureous green Precipitate, crystallized Salts, and sulphureous Fumes with Oil of Vitriol. From hence this learned Author concludes, that this Substance contained the active sulphureous Parts of the Wood, blended with more active igneous Particles. These, united with the alkaline Salts, make a kind of Soap, or sulphureous saponaceous Salt, resembling Soap of Tartar, or *Hepar Sulphuris*. The crystallized Salts he attributes to the Acid of the Wood, mixing with the alkaline Salts. All these Parts of the Wood then are contained in our Pot-ash; and he observed the same in the common *Soda*, or *Cineres clavellati*; altho' they are in a less Degree in that than in the *Russian* Pot-ash.

Besides these, he shews that Pot-ash contains a metallic Substance, which affords the *Prussian* Blue. We may add further, that the Combination of these Principles makes many Properties in Pot-ash, more than what result from them in a State of Separation. The most remarkable of these seems to be its explosive Quality; which we take to proceed from the crystallized Salts approaching to the Nature of Nitre, and uniting with the Sulphur and Charcoal; by which they form, from all these Ingredients of
Gunpowder,

Gunpowder, a kind of that explosive Substance, whose Parts are highly rarefied in an intense and confined Heat, by which they readily explode in boiling Lye.

By this we may perceive, that the Difficulty in making Pot-ash aright, is, first, to reduce the Materials to Cinders and Ashes, and at the same time to preserve their volatile, sulphureous, and exhalable acid Parts, that are totally destroyed in such a Degree of Heat; and, secondly, to calcine these Ashes still further, so as to flux their Salts, and vitrify their terrestrial Parts, and at the same time to keep them separate from each other, or prevent their running into an indissolvable Glass. To give Pot-ash some of these Properties, seems plainly to require a Degree of Heat that will totally deprive it of others.

The most likely Way by which it comes to receive all these Properties, is from the Way of making it in *Sweden* above described. In that Process, the green Fir, in which the Ashes are burnt, impregnates them with the acid saline Parts of the Wood or Tar, which is well known to be in pretty large Quantities, and is absorbed and fixed by the alkaline Salts, and porous terrestrial Parts of the Ashes in this Process; so that, besides the fixed alkaline Salts of the Ashes, the Pot-ash, thus made, must likewise contain the more volatile Salts of the Pine, which are exhaled in Smoke by burning the Pine alone in the open Air. Besides these, it likewise contains the resinous Parts and sulphureous Fumes of the Pine, that are hindered from exhaling by the Heap of the Mass.

As

At the same time the alkaline Salts are fluxed in the open Fire, and in a manner vitrified with the terrestrial Parts of the Ashes, which gives them their hard and solid Consistence; whilst the sulphureous and acid Parts of the green Wood hinder them from turning to a perfect Glass, or inert *Calx*. All these Parts united together in the Fire, make that saponaceous Substance we find in the Pot-ash thus made, which further hinders the Vitrification of the Mass, and endows it with many of its most peculiar and active Properties.

From hence we may see how difficult it is to make a Substance endowed with all these Properties in any other manner. This is the Reason why we could never before make Pot-ash equal to that of *Russia*, and the other Northern Countries, altho' we have much greater Plenty of Materials and perhaps better: For this Way of making it has never before been thought of by the Learned, or practised anywhere else, as far as I can learn.

Somewhat of the same Qualities are communicated to the *English* Pot-ash, by the Way of making it above-described; but in a Degree as much inferior, as dry Straw, used for that Purpose, is to green Wood: Accordingly our Workmen find that Pot-ash as much inferior to the foreign, for many Purposes.

From this Account of the Contents and Qualities of Pot-ash, and the Way of making it, we may form some Judgment of the other Ways of making it, proposed by Authors, and suggested by many. Thus *Lemery* and others tell us, Pot ash is made in *Russia*, and all the Northern Countries, only by

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calcining the Ashes in Pits brick'd within, and sprinkling them well with Lye, till they become hard and solid. But such a Calcination of Ashes with a lixivial Salt, must render them whiter, instead of black, and must further destroy the active sulphureous Parts of the Wood, which we find in Pot-ash rightly made. So that this only leaves the Ashes in the State they were at first, or turns them into a kind of indissoluble Glass, as we have found upon Trial.

This, and the like Mistakes about the Way of making Pot-ash, seem to proceed from a general Error concerning the Nature of it; for it is commonly supposed to be only a kind of inert *Calx*, impregnated with nothing but a lixivial Salt. Some such Mistake seems to have frustrated all the Attempts hitherto made of making Pot-ash in *America*; for, upon Trial, what they have made there was found to be no better than common Ashes.

But the most general Mistake about the Way of making Pot-ash, seems to proceed from the Accounts we have of making it, from Glaswort, and some marine Plants, which are said to be easily converted to this kind of Substance, in the manner above-mentioned. But we apprehend, the Way of making it from Wood must be very different: For these Herbs are easily reduced to Ashes by a small Fire, that does not intirely consume their sulphureous Parts, which Wood is not. These Ashes abound with a great Quantity of alkaline and some neutral Salts, that readily convert them to a hard and solid Consistence, which Wood does not. They have likewise few or no terrestrial Parts, to run them into an indissoluble Glass, when fluxed in the Fire,

as happens in Wood-ashes. Besides, these Herbs have few or no sulphureous or acid Parts, like most Woods; and the Pot-ash made of them has few of these Principles in it, like what is made of Wood.

It is however generally said, if we burn our Wood in a close Place, as a Kiln in which we burn Lime, or make Charcoal, or a Pit dug in the Ground, we may impregnate the Ashes with the sulphureous Fumes and acid Parts of the Wood, only by the Closeness of the Place, or by smothering the Fire in it. If at the same time we impregnate them with a greater Quantity of lixivial Salt, it will flux the whole Mass, and make it run into a solid hard Consistence like Pot-ash. This is commonly directed to be done, by throwing fresh or green Wood or Herbs upon the others, as they are burning, before they are quite reduced to Ashes; or by smothering the Fire, as in making Charcoal; and at the same time to sprinkle the Ashes, thus burnt, with a strong Lye from time to time, in the manner commonly practised with Glaswort.

This would be a more ready Way of making Pot-ash than any of the above-mentioned; but as those who give their Advice about it, have neither tried it, nor seen it done; and those who have tried this or any other Way, find more Difficulty in it, than they at first imagined, we shall suspend our Judgment about it, till we see it fairly tried, lest we should deter some from making useful Experiments of it, or lead others into fruitless and expensive Attempts.

By the various Ways of making Pot-ash above-mentioned, and the different Materials it is made of, there appear to be many different Kinds of it.

that have as different Qualities. It would lead us too far beyond our present Design, to give a particular Account of each of these; but as they are used in many of our Manufactures, it seems worthy Inquiry, to know what Sorts are generally used, and what are the fittest to be used in them.

The Workmen in *England* make two general Kinds of it, which they distinguish by the Names of Pearl-ash and Pot-ash. The first is a mere lixivial Salt, which is supposed to be the only Ingredient of any Efficacy in Pot-ash; but, upon Trial, there is found to be a great Difference between them, especially in making Soap. The Salt is so weak in the Pearl-ash, that it does not intirely dissolve and unite with the Fat. The Reason seems to be, that these Salts are dissolved in Water, in order to extract them, by which they lose many of their caustic igneous Parts; whereas in Pot-ash, the Salts are calcined and fluxed in an open Fire, with the ignited terrestrial Parts of the Ashes, which makes them more sharp and corrosive: They are likewise incorporated with the Coal, and fuliginous Parts of the Vegetables they are made of, or with the resinous Parts of Fir, which gives them the sulphureous Quality above mentioned, and makes a kind of Soap of Tartar, or *Hepar Sulphuris*, in all Pot-ash; which makes these Salts so ready to dissolve, and incorporate with Oil, or other pinguious Substances.

This is perhaps the Reason, why the *Cineres Russici* are ordered for this Purpose, instead of a mere lixivial Salt, by the College of Physicians in their late Dispensatory. The Soap made of them must be impregnated with their heating sulphureous Quality,

Quality, which will make it more aperient and detergent, but not so mild and soft as some others; by which it may be more fit for obstinate and indurated Obstructions, but will be more offensive to the Stomach; which is much complained of by some People, who take large Quantities of the sharper Kinds of Soap.

But, to consider Pot-ash as a Commodity in Trade and Manufactures, which is its chief Use; it appears, that the People in *England* not only have it at a dear Rate, but the worst Sorts of it, at least for most Purposes; which cannot but have a proportional Influence on their Manufactures: For it is generally of as great, and some Sorts of a greater Value in their Markets, than a pure lixivial Salt; notwithstanding the small Quantity of such Salt in Ashes, and the Trouble and Expence of extracting it; which seems to be occasioned by their not knowing how to convert Ashes into this Commodity; for in *Sweden*, where this Art is known, *Lundmarck* tells us, Pot-ash is sold for little more than a Farthing a Pound, which costs our Workmen nigh Six-pence.

But this is not the only Inconvenience we labour under for want of this Commodity; the Sorts we are chiefly supplied with are perhaps the worst of any, and unfit for many Purposes for which Pot-ash is used. The only Pot-ash almost to be met with here, comes from *Russia*, *Sweden*, and *Dantzick*, or is made in *England*. These are all made either of Wood or Fern-ashes, whose Salts are never so pure and white at the best, as some others: But, by the Way of making them, and the Experiments on them above-mentioned, they

they appear to be impregnated with Coal, Smoak, and Soot, which renders them still more foul and impure, makes them of a black, brown, or green Colour, and of a peculiar sulphureous Quality. On this account they are intirely unfit for making white Glass: They make a very coarse and strong kind of Soap; they are too foul, sharp, and corrosive for bleaching, and are as unfit for dyeing, at least many Colours.

It is perhaps for this Reason, that the Workmen here, as they shewed me themselves, make all their white Glass with Salt-petre; which must not only be more costly, but *Neri*, *Merrett*, and others, tell us it is not so good, at least for the better Sorts of Glass, as a sharper lixivial Salt. What they use for dyeing I am not so well apprised of: It is said, they use the volatile Alkali of Urine; but the *French* Pot-ash, made of the Lees of Wine, is generally allow'd to be the best for that Purpose. So likewise the *Alicant* Pot-ash is reckoned much the best for bleaching, and making of Soap; as the *Syrian* and *Egyptian* is for making Glass.

These purer Kinds of Pot-ash are all made of Herbs, that grow only in the more Southern Climates, whose Salts are finer and whiter, and less acrid and corrosive than the Salts of Wood, or most other Vegetables; and by the Way of extracting them by Calcination in a more open Fire, they are more free of Coal, Smoak and Soot, or any other heterogeneous Mixture. On this account they are much better for the Purposes above-mention'd, than the coarse and foul Kinds of Pot-ash that our People are supplied with.

All we have of these Kinds of Pot-ash, it seems, comes only from *Spain*; for which Reason our People were obliged to petition to allow the Importation of Pot-ash from thence, during the late War; as appears by an Order of the King and Council of the 24th of *June* 1742, since they could not do without it in many Manufactures: So that it may be worth our Inquiry, to know what it is that produces so necessary a Commodity.

This Kind of Pot-ash is commonly called *Barrilha*, from an Herb of the same Name in *Spain* that produces it. The first Account we have of this *Barrilha* is from *Amatus Lusitanus*, who leaves us much in the dark about it. It is generally said in *England* to be a Plant pretty well known to the Botanists by the Name of *Ficoides Neapolitana, flore candido*. *Hort. Lugd. Bat.* but for what Reason I cannot say. We have as little Reason to believe with *John Baubine* that it is what he calls *Kali vulgare*: For Mr. *de Jussieu* has shewn us, that the true *Barrilha* is a different Plant from any of these, from his own Observations of it in *Spain*, where it was cultivated; of which he has given us a particular Account, by the Name of *Kali Hispanicum, supinum, annuum, Sedi foliis brevibus*. *Mem. Academ. Anno 1717. p. 93. or Alicant Glasswort.*

The Pot-ash made of this Plant, he tells us, makes the best Soap, the finest Glass, and is the best for bleaching of any other; for which Reason it is much sought after in all Countries, where they value themselves for these Manufactures. But I question very much, whether our Workmen have it either pure and.

and genuine, or in sufficient Quantities for these Purposes: All the Use I find made of it among them, is to make hard Soap; altho' they say what they have of it spoils their soft Soap, by making it curdle. This is well known to be the Effects of Sea-Salt; and Mr. *de Jussieu* and others tell us, that the true *Barrilha* is often adulterated with Sea-weeds, which contain such a marine Salt; so that it is probably only this adulterated Sort that they have. Accordingly, all the *Barrilha* I have found here, was of a dark-brown Colour, and very foul and ponderous; whereas the true Sort is said, by all who know it, to be more porous, pure, and of a bluish Colour. It is for this Reason in all Probability, that, notwithstanding all the *Barrilha* our Workmen have at so dear a Rate from *Spain*, yet they can never make so good Soap, as what comes from thence, and some other Places.

The only Way then, by which we are likely to have this Commodity either pure and genuine, or in sufficient Quantities at a reasonable Rate, is from the Herb itself that produces it. Whether or not it would grow in *England* is not known, as I believe it has never been tried: But there is no doubt but it would grow very well in our Colonies in *America*, as I am certainly informed it does in the *Spanish* Colonies there, where they have great Plenty of it; and a Sort that is indigenous, particularly in *Peru*, which might probably be found in our Colonies, if sought for by those who knew it. But wherever it will grow in any of the *English* Dominions, there is no doubt but it would be a considerable Improvement, where Pot-ash of all kinds is so valuable a Commodity,

Commodity, and so much wanted; for it grows on the same Ground with Corn of any kind, which it does no Harm to, as it is a small annual Herb, that does not spread till the Corn is ripe, or off of the Ground.

There are some other Plants that are known to make a kind of Pot-ash, commonly called *Rochetta*, which is said to be even preferable to the *Barrilha*, especially for making Glass. These are the first and second Kinds of *Kali*, described by *Prosper Alpinus*, in his Account of the Plants of *Egypt*. The first of which is the above-mention'd *Ficoides* that grows in *Italy*, and all over the *Levant*, but the other is peculiar to *Egypt*. These would be fit Improvements for our Colonies in *America*, where we seem to want nothing more than some proper Production for the vast Tracts of Land we are possessed of there. But these Plants alone afford a Commodity, which *Pr. Alpinus* and *Rauwolfius* tell us they saw many large Ships yearly loaded with in *Egypt*, and which gives the Excellency to the Glass and Soap that are made at *Venice*.

It would be worth while then at least to make a Trial of a Production, that is likely to improve both our Trade abroad, and our Manufactures at home. It was this that put me upon the present Inquiry, as an Improvement fit for our Colonies, which if I find acceptable, I shall hereafter consider some others.

XI. *A Letter from Dr. Laurence Garcin, of Neuchatel, F. R. S. to Sir Hans Sloane Bart. late P. R. S. concerning the Cyprus of the Ancients : Done from the French by W. Watfon, F. R. S.*

S I R,

Read Nov. 24.
1748.

THE Plant in Question is a Shrub, which varies considerably in its Size and Figure, according to the Nature and Soil of the Country where it naturally grows, as well in *Asia* as in *Africa*, where this Plant is much used, both as a Medicine, and for its agreeable Odour.

Our Author has given us the true Characters of the Fructification of the *Cyprus*, after the Method of the celebrated *Linneus*.

1. Its *Calyx* is an expanded monophyllous Cup, cut into four Lobes, pointed at their Extremities, and continuing attached to the Fruit.

2. Its *Corolla* consists of four oval Petals, somewhat pointed and sinuous. They grow distant one from the other, and are placed between the Lobes of the *Calyx*.

3. It has eight erect *Stamina*, ranged two by two almost horizontally, and parallel to the Sides of the Petals, and surpass them in Length about half a Line. They grow from the Base of the Embryo at a little Distance one from the other, and arise diminishing in their Bulk to their Extremities. Their *Antheræ* or Summits form each of them a little kind of Purse.

4. Its

4. Its *Pistillum* is round, and occupies the Middle of the *Calyx*. Its Style is erect, and terminated with a pointed *Stigma*. Its Length somewhat exceeds that of the *Stamina*.

5. Its *Pericarpium* is a round dry Capsule, slightly four-corner'd; each of which Corners has a small Prickle. It is divided into four Compartments by an extremely delicate Membrane, arising from a *Placenta* which occupies the Centre of the Capsule.

6. Its Seeds are small and numerous: Each of them is pyramidal, and somewhat quadrangular, of which the Point is sometimes streight and sometimes crooked. Every Seed is fastened by its Point to the *Placenta*, as to a common Centre, and their Bases are sustained by the Sides of the Capsule, all the Cavity of which is filled by them.

There is but one Species of this Shrub generally known through all the East; and this is subject to vary according to the Climate, the Season, and the Soil.

Its Names are,

1. According to different Nations.

Greek, <i>κύπρος</i> .	Apothecaries, <i>Alcanna</i> .
Latin <i>Cyprus</i> .	The People of } <i>Mail-An-</i>
Hebrew, <i>Copher</i> .	Malabar, } <i>schi</i> .
Arabian and } <i>Henna</i> .	The Brachmans, <i>Mety</i> .
Perfic }	Malayans, <i>Dann Lacca</i> .
Egyptian, <i>Elbanne</i> .	Javans, <i>Batschiar</i> .
Italian, <i>Alchanna</i> .	Chinese, <i>Tsingka Hou</i> .
Spanish, <i>Alkenna</i> .	Indians, <i>Inne</i> .
The Portuguese } <i>Foula</i>	At Bengal and } <i>Mendi</i> .
in the Indies, } <i>Aybana</i> .	Surat, }

2. According to Authors.

Ligustrum Dioscoridis. Matthiol. 117.

Ligustrum Egyptiacum latifolium; item angustifolium. C. Bauh. Pin. 476.

Ligustrum Egyptium. Joh. Bauh. T. 1. p. 532.

Ligustrum orientale. Park. 1447. Raii Hist. 1603.

Rhamnus Malabaricus, fructu racemoso caliculato.
Raii Hist. 1573.

Its DESCRIPTION.

The *Cyprus* grows generally as a Shrub of ten or fifteen Feet in Height, and has very much the Appearance of Privet.

Its Trunk grows sometimes as thick as a Man's Thigh, is sometimes streight and sometimes crooked, and produces a great Number of Branches irregularly. Its outward Bark is ash-colour'd, and much furrow'd, and detaches itself from the Trunk of the Tree in long Scales or Pieces, by the Heat and Dryness of the Climate, as in the *Persian* Gulf. Its inward Bark is reddish without, and whitish within. That of the Branches is smooth and red, like that of the Hazel-tree, and green within. Its young Branches are streight, flexible, and moderately long. The Wood of the Trunk is hard and whitish.

Its Leaves are disposed in different Orders upon the same Twig. Sometimes they are placed opposite in Pairs along the small Branches, and this most generally cross-wise; sometimes by three and three; but then the Leaves are less, and this Disposition generally takes place in the larger Branches; sometimes they are alternate, but rarely, and then the Leaves are largest. The least Branches are most charged with Leaves, the larger ones least. All
these

these Leaves are pointed at each End; the largest are two Inches long, and about an Inch broad in their Middle; the smallest bear half the Dimensions of the largest: Their Edges are even: They are smooth, shining, and of a beautiful green Colour: Their middle Rib, which serves to each Leaf as a short Pedicle, is terminated in their Point, but sends out, in its Passage through the Leaf, alternately four or five nervous Filaments on each Side. These Leaves are much like those of Privet.

The Flowers grow in Bunches at the Extremities of the young Branches, and are endowed with a very agreeable and singular Odour. They are of a Straw-colour; but as they grow old and wither, they become of the Colour of a Citron. The *Calyx* is more pale than the *Corolla* of the Flowers. Its Petals are turned up as much if not more than those small Petals are which adorn the Centre of a double Rose. The *Stamina*, which are white, transparent, and which grow from the Base of the Embryo of the Fruit, form as it were a double Cross, by their almost parallel Situation and Extension between the Petals. The Lobes of the *Calyx*, being of the same Length and Form of the Petals, seem to give to the intire Flower an octogonal Figure. The Summits or *Antheræ* are small, and of the same Colour as the Petals, each having a deep Furrow in its Bottom; the more these decay, the more yellow they grow, in the same manner as the Petals. The Furrow in the *Antheræ*, which at first is of a palish black, grows of a deeper Hue, as the Flower fades: The *Pistillum*, after the Flower is gone, grows larger in the *Calyx*, and becomes, when perfectly ripe, a dry, membranous, round Fruit, of about three Lines in Diameter. But before
it

it arrives to this State, it resembles very much a fleshy Berry, green on one Side, purplish, and sometimes black on the other, with very little Juice. This false Berry is the growing Capsule, the Side of which is soft, succulent, and very thick; which, in proportion as it increases, becomes thin, membranous, dry, and brittle: In becoming thus capacious and thin it gives Room to a large Number of pyramidal Seeds, very close one to another, and fastened all by their Points to a common Center, a kind of *Placenta*. When this Capsule is in its Perfection, its Outside is shining, and not unlike the Seed of Coriander in Colour. The *Pericarpium* is as it were divided into four *Loculi*, by Membranes so delicate, that they must be regarded with great Attention, to be satisfied of their Reality. The exterior Form of this Fruit sufficiently shews this Division, by its Roundness being interrupted by four slight Ribs, like those of a Melon, which shews as many Cells. The Membranes, which divide these Cells, arise from the *Placenta*, and are inserted into the Sides of the Capsule.

The Seeds, which fill all the Capsule, amount to about four or five dozen, according as they are more or less nourished; because the larger ones receiving more Nourishment, make the smaller ones abortive. They are always so pressed in their Apartments, that their pyramidal Figure is owing only to this Pressure, which arises from their reciprocal Increase. The pyramidal Points of these Seeds are crooked in some, and bent in others, according to the Direction given them in their growing. Their Colour is red or brown, and always somewhat shining.

Remarks.

REMARKS.

We find, in the ancient Writers of Plants, such as *Theophrastus*, *Dioscorides*, and *Pliny*, who have all in their manner treated of Vegetables, of how much Esteem the *Cyprus* was among the Ancients. The Historian *Josephus*, and St. *Jerome* have mention'd it as a rare and precious Plant, placing it in the same Rank with the most valued Spices. The fine Smell, which its Flowers send forth in the Countries where they grow naturally, as in *Egypt*, *Syria*, *Arabia*, *Persia*, &c. has occasioned its Use in the earliest Time; and the same Use continues in those Countries. Its being twice mentioned in * *Solomon's Song*, is a very great Proof of its being much valued in the most ancient time. We there see it was accustomed to be cultivated even in their Vineyards. The Perfumers in old times made thereof an Oil or precious Ointment for various Uses; but principally to give their Anointings a grateful Odour, and to make supple the Limbs of the Body.

Modern Authors have given themselves great Trouble to be thoroughly satisfied of the History of this Plant. There have been great Controversies among them concerning it in endeavouring to settle its Description; but it must be confessed they have made a very small Progress in discovering to us its true Characters. How many Mistakes have the Botanists

* *Solomon's Song*, chap. i. v. 14. ch. iv. v. 13. In both these Places the *English* Translation of the Bible has it *Campfire*, instead of *Cyprus*.

stanist of the two last Centuries made, owing to the bad Descriptions of this Plant, which the Ancients have left us.

Dioscorides, who, by describing the Plants he treats of too briefly, always leaves their Characters imperfect, says (perhaps after some other Author more ancient than himself) *that the Leaves of the Plant in Question are like those of the Olive tree; that its Flowers are in Bunches, and that its Fruit is black, like that of Elder.* This was enough to make the *Latins* conjecture, that the *κύπρεος* of this Author was the *Ligustrum* or Privet; and the more so, as the *Cyprus* was intirely unknown to them, since it only grew in *Egypt* and in *Syria*, where it was always called *Henna*, or *Alhenna*, and, by Corruption, *Alkanna*.

There is some Appearance, that, as the *Greeks* received a good Quantity of this Drug from the Isle of *Cyprus*, as a Species of Merchandize, they would chuse to call it *Cyprus*, rather than give it any other Denomination, on account of the Quantity furnished to them from the Isle of that Name. *Pliny* took it first for a kind of Privet of *Ligustrum*, which grew particularly in *Egypt*, and afterwards he thought it to be the common *Ligustrum* or *Europe*: This shews how uncertain he was as to the Plant in Question. He judged ill in comparing the Fruit of the *Cyprus* with that of the Jujube tree; but was more happy in likening the Fruit (Capsule) to that of the Coriander, as they agree in Colour, though that of the *Cyprus* was more large. *Matthiolum*, who thought himself greatly above his Cotemporaries in the Theory of Plants, asserts boldly, that our Plant was the common Privet:

And

and in this he thinks himself justified, not only from the Description of *Dioscorides*, but from the Virtues attributed to the *Cyprus* by *Pliny*. He even ridicules those who think that the *Ligustrum* and *Cyprus* are different Plants. *Fuchsius*, who wrote before *Mattiolus*, had nevertheless Reason to believe them of a different Genus, by the Account given of the *Egyptian* Plant by *Pliny*; but he was wrong in confounding it with the *Phillyrea* of *Dioscorides*, and in this Mistake has been followed by *Dodoneus*.

Bellonius, who had seen this Plant in its Place of Growth, well knew that it was not the *Ligustrum* or Privet: He saw also how the Commentators of the *Arabian* Authors were deceived in taking it for such.

Rauwolf and *Prosper Alpinus*, who met with it in their Travels, after having observed it in the Places of its Growth, believed, as *Pliny* had done, that it was a kind of *Ligustrum*, which approached very near to that of *Europe*. They have each of them given a different Figure; which made *Caspar Bauhin* believe that there must be two new Species of *Ligustrum*; but herein he was not follow'd by the ingenious Mr. *Ray*. In fact, we ought to acknowledge, by the Characters here set down, that our *Cyprus* is of a Genus truly different, and the only one of its kind.

The *Hortus Malabaricus* has given a Figure of this Plant under the Name of *Mail-Anschi*, which represents the End of a large Branch ill-chosen, and somewhat wither'd, without doubt by the Fault of the Designer, who has drawn it in its natural Size; which is greater in *Malabar* than elsewhere, because

F f f f

of

of the Rains which fall there in Abundance half the Year. This Shrub is less in all its Parts in *Arabia*, and to the South of *Persia*, because in those Countries it rains seldom; but, in Recompence, its Flowers have much more Smell than in *Malabar*. It must be remarked here upon this Occasion, that the Description just now given, and which contains the Size of the Parts, was made in a Garden in the *Persian* Gulf belonging to the *Dutch* Factory, and situate about a League from the Town of *Gameroon*, otherwise called *Bender-Abassi*, where there was one of these Trees carefully preserved, which was the first I saw in the *Indies*; as it was complete in all its Parts, having Flowers and Fruit; and as it appeared to me agreeable and curious, especially on account of the fine Smell of the Flowers, and as it was a new *Genus* to be established in Botany, I examined it with great Exactness, and noted its Characters, Figures, and Dimensions. I did not conceive it to be the *Cyprus*, not then knowing what it was. I asked the People of the Country the Name of this beautiful Shrub: They only called it *Henna*, and I could learn no other Name: They assured me it had no other Name, either in *Persia*, or in *Arabia*. It was on the 1st of *December* 1721. that I observed it, and described it under the Name of *Frutex Persicus, foliis Ligustri, flore et fructu racemoso, Henna vulgo dictus*. I thus characterized it, in Expectation of finding it, if it had already been described among Authors, after my Return to *Europe*. When I returned in 1730. I had the Satisfaction to find it in Mr. *Ray*'s History, by the Description which he has given of it, extracted from various Authors, in the
Chapter

Chapter of *Ligustrum* under the *Synonyma* of *Parkinson*, and to see it in the other Authors I have mention'd, especially the Figure given by *Rauwolf*, which is not a bad one, and which is copied by *Clusius*, *Dodonæus*, *Parkinson*, and *Dalechamp*.

The Figure in the *Hortus Malabaricus* under the Name of *Mail-anschi*, does not so happily represent our *Cyprus*, as that excellent Work generally does the Plants it treats of. The Leaves of this Plant there are half-wither'd, and not in their natural Disposition. *Rauwolf's* Figure is much nearer the Truth. The Flowers are not much better represented than the Leaves, in the *Hortus Malabaricus*; as, besides other things of less Moment, the Authors of that Work have neglected to make the Petals appear between the Lobes of the *Calyx*, as always happens in a natural State; by which Disposition the Flower appears of an octagonal Figure. *Rumphius*, who has written an History of the Plants of *Molucca*, has given a Description of this Shrub, not different from mine.

By what is here laid down of the Characters of this Plant, we plainly see that it differs widely from the *Oxyacantha* and *Rhamnus*; of one of which the Authors of the Notes to the *Hortus Malabaricus* suspected the *Cyprus* to be a Species. This occasion'd Mr. *Ray* to range it under the last, supposing its Fruit to be a Berry, which nevertheless it is not. This learned Author moreover could not think that the *Mail-anschi* was the *Cyprus*, because of the Difference in the Descriptions among Authors, and of the Imperfection of those of *Rauwolf* and *Alpinus*.

Rumphius, just now quoted, has ill compared the

Colour of the Leaves of *Cyprus* to those of the Olive-tree.

This Shrub, so cherished among the Eastern Nations, is cultivated in *Africa*, *Asia*, and all the *Indies*; that is to say, from near the Equinoctial even to 35 Degrees of North Latitude; where it is much used, as we shall find by the great Commerce caused thereby in the *Levant*, according to the Relations of Travellers of Credit.

This Plant does not love Shade, even under the Torrid Zone, because of the violent Rains there at the time of the Western *Monsoon*, no more than it does in cold Countries, our Author means those of the fifth Climate; but towards the Tropick, and even in *Arabia*, it grows best when a little shelter'd from the Sun. In hot and dry Countries, as in the *Persian* Gulf, where I first saw it, it produced a great Number of Boughs and Branches very short, which gave it the Appearance of White-thorn. On the contrary, towards the Equator, its Branches are further from each other, and longer, occasioned by the Moisture from the Rain. The Back splits into Scales, and detaches itself in Pieces from the Trunk, in those Countries where it rains seldom; but in *Malabar*, in the Isles of *Ceylon* and *Sunda*; the Back continues intire and united almost all the Year, because of the Moisture of those Places.

Rawwolf remarks, that the *Turks* and *Moors* cultivate this Plant with Care, and even keep it in Pots, on account of the Smell of the Flowers, which somewhat resemble Musk. They keep these Pots in Winter in Chambers or Caves to preserve the Plants from Cold.

Our

Our Author forgot to remark one Circumstance, mentioned by *Bellonius* in the first Book of his Observations (*Chap. 44. apud Clusium*), where it is said, that the *Henna*, or *Alchanna*, which is our *Cyprus*, differs from Privet, because the Leaves of Privet fall, and those of *Cyprus* continue all the Year. But this Observation is of no Weight, because this Difference is only apparent; and it is certain, that if our Privet was cultivated in *Egypt*, its Leaves would not fall off in Winter, because it is not there sufficiently cold.

The Uses of Cyprus.

Bellonius, who was the first of the Moderns who treated of this Shrub under the Name of *Alcanna*, and spoke of its Culture in *Egypt*, tells us, that the Powder of its Leaves is so great an Article of Commerce among the *Turks*, that they load several Vessels from *Alexandria* for *Constantinople*, where the Sale of it is so great, that the Grand Signor's Revenue therefrom amounts yearly to 18000 Ducats. According to him, the great Consumption of this Powder arises from its being used in beautifying the Skin and Nails, in making them red with a Decoction made therewith. The Women, he says, generally use it all over *Turkey*, to dye the Skin of those Parts which are from the Navel downwards, as well as their Hands and their Hair. Their Children are served in the same manner. They consider this as a great Ornament; and that the Colour may hold longer, and penetrate deeper, they apply it usually when they go out of the Baths. This
Practice

Practice of dyeing, to beautify the Body, is extended even to their Horses, of which they tinge the Mane, the Tail, and the Hoofs. They often add Alum to heighten the Colour. This Powder is sent from *Constantinople* to *Russia*. Let us now consider the other Properties of *Cyprus*.

It is not necessary here to take notice of what *Dioscorides* and *Pliny* attribute to this Plant; they may be consulted, if, at the same time, they are regarded as being very little skilled in its true Qualities. Our Author contents himself with saying, that the *Persians* and *Arabians*, who appear to have been anciently the first that used this Plant, frequently use at present not only its Flowers to perfume their Linen, their Cloaths, and their Tables, but make a greater Use of its Leaves in a Decoction, for the Cure of all Distempers of the Skin, as the Itch, Scabs, and Ring-worm, which the Air of their Country causes from its Heat, and from the Drought which often reigns there to a great Degree. These Disorders, if they are neglected to be cured as soon as possible in dry Climates, easily degenerate into the Leprosy; and it is on account of these Disorders of the Skin, that the eating of Pork is forbidden to People of every Religion in these Countries; because that Food there is known to occasion these Distempers.

All the Nations of the *East Indies* make use of it in Medicine, for the same, as well as for several other Disorders; but they particularly use the Leaves to dye their Nails; which our Author thinks they had originally from the *Arabians*. In dyeing their Nails, the *Indians* make use of the fresh Leaves,
which

which always grow in great Plenty in their Gardens, and apply them beaten upon their Nails, mixing with them sometimes a little Lime and Juice of Citron. This Colour lasts a great while upon the Skin, on account of Sweating. A strong Decoction of the Leaves in Water is sometimes used to tinge their Nails, but more generally their Skin and Hair.

There is Reason to believe, that this pretended beautifying of the Skin, the Hair and Nails, which long Custom has established among the Eastern Nations, owes its Origin to a quite different Principle than that of beautifying. The Ancients had no other View in the Beginning, than the Prevention of pruriginous and leprous Disorders in the Skin, to which their Climate subjected them, as well as to preserve them from Vermin, as the Leaves of *Cyprus* have that Property. But as in using Baths with these Leaves therein, they dyed their Skin either red or yellow, according to the Preparation, they accustomed themselves to this Colour by degrees, and afterwards regarded it as a salutary Embellishment.

These Baths, which there are constantly employed for the Cleanliness and Health of the Skin, and which the Necessity of using has established as a Point of Religion, and a Duty, for the better Prevention of these Maladies, is certainly a true Method to preserve as well the Body as the Skin in a good State. These good Effects are extended further by using the *Alcanna*; because its Colour, passing in the Opinion of these People for a necessary Ornament, and a Mark of Cleanliness, makes the Practice of bathing better observed.

It

It seems to our Author, that these Remarks should be communicated, as well as the Characters and Description of the Plant in Question, to render its History more complete, and by these means to make it known; to the end that the Curious may form some Opinion of the great Praises which the Ancients have bestowed upon this Plant. I am

S I R,

Your, &c.

Laurence Garcin.

ERRATUM.

In Page 541, l. 22, for *Art. XI.* read *X.*

PHILOSOPHICAL TRANSACTIONS.

For the Month of *December*, 1748.

- I. *An Account of an antient Shrine, formerly belonging to the Abbey of Croyland; by Wm. Stukely M. D. Coll. Med. Lond. Soc. & Eccles. D. Georgii Martyr. Lond. Rector.* p. 579
 - II. *An Eclipse of the Sun, July 14. 1748. observed by the Right Honourable James Earl of Morton, Mr. le Monnier, Royal Astronomer and Member of the Royal Academy of Sciences at Paris, and Mr. Ja. Short, Fellows of the Royal Society.* p. 582
 - III. *A Letter from Mr. David Erskin Baker to Martin Folkes Esq; Pr. R. S. containing Considerations on two extraordinary Belemnitæ.* p. 598
 - IV. *A Letter from Benj. Cooke F. R. S. to Peter Collinson F. R. S. concerning a mixed Breed of Apples, from the Mixture of the Farina.* p. 602
- V.

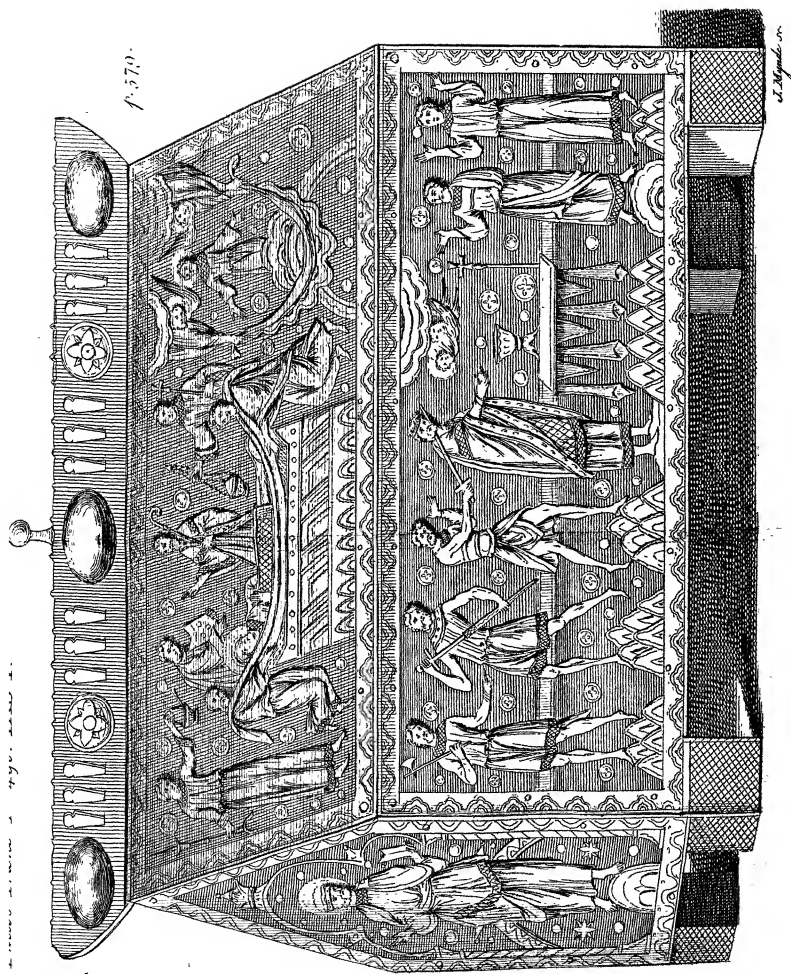
The CONTENTS.

- V. *A DESCRIPTION of the town of SILCHESTER in its present state. With a short ACCOUNT of an antient DATE in Arabian figures at Walling near Aldermarston in Berkshire.*
By J. Ward, F. R. S. and P. R. G. p. 603
- VI. *A Summary of some late Observations upon the Generation, Composition, and Decomposition of Animal and Vegetable Substances; by Mr. Turbervill Needham F. R. S.* p. 615
- VII. *Observationes astronomicæ variæ factæ in Paraquaria, Regione Americæ Australis, ab anno 1700 ad annum 1730. quas cum Regali Societate communicavit Jacobus de Castro Sarmento M. D. Coll. Lond. Lic. & R. S. S.* p. 667
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ERRATA.

Nº. 486, p. 232, for *Durobrovæ* read *Durobrivæ*. *Ibid* p. 233, *Henry Wharton* is said to have been always *infirm and sickly*: But in his *Life* written by *Bishop Green*, and printed before his *Sermons*, it is said, *His Constitution was vigorous and healthy*.

Printed for C. DAVIS, over-against *Gray's Inn Gate*
in *Holbourn*, PRINTER to the ROYAL SOCIETY;
M.DCC.L.



*A most ancient Shrine, of curious enamelled work, on Copper, in Colours.
12 inches long $10\frac{1}{2}$ high $4\frac{1}{2}$ broad.*

— — — — —

- I. *An Account of an antient Shrine, formerly belonging to the Abbey of Croyland; by Wm. Stukely M. D. Coll. Med. Lond. Soc. & Eccles. D. Georgii Martyr. Lond. Rector.*

Read Dec. 8.
1748.

THE Shrine before us is a great Curiosity. Few of this kind of Antiquities escap'd the general Ravage of the Dissolution of Abbeys: For which Reason I thought it would be an agreeable Amusement to the *Society* to have a View of it; and to preserve a Drawing of it as in TAB. I. and II. The Shrine is made of Oak, plated over with Copper, upon which the Figures are chased in Gold: The Ground is enamel'd with blue; in the Ridge along the Top are three oval Chrystals set transparently; its Dimensions are as expressed under the Print.

Mr. *Eayre* of *St. Neots* sent it to me to have my Opinion of it. It was found in the House of a Gentleman of that Neighbourhood, who never shew'd it during his Life-time; and who possibly might have given us some Account, as to the History of it; and at present we have no means left of finding it out, but by Conjecture.

I conceive it came from *Croyland-Abbey*. There was an Intercourse between this Abbey and *St. Neot's* Priory; insomuch that *St. Neot's* Body was carried hence to *Croyland-Abbey*, and inshrined there.

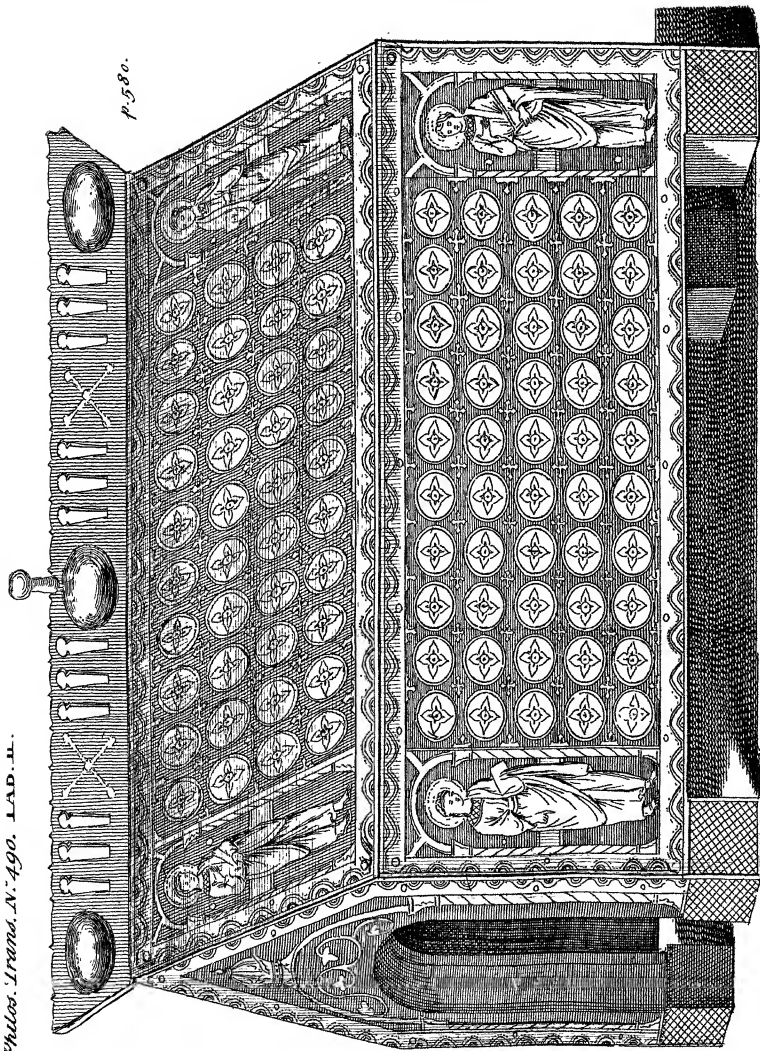
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These

These Shrines were made for receiving Reliques of Saints, in old Abbeys, Churches, and Cathedrals. (See a Print in the History of *Canterbury*, and in *Dugdale's Monasticon*, of the high Altar of the Church of *St. Augustin* there; no less than thirteen of these Shrines standing around). These were carried about in Processions on their anniversary Days; sometimes embellished with Jewels of inestimable Value. Besides these portable ones, there were others, built of Stone, Marble, and other Materials; like that of *St. Edward the Confessor* in *Westminster-Abbey*; one now in *Chester Cathedral* of *St. Werburga*, whercon the episcopal Throne is set, adorn'd with Sculptures of *Saxon Kings* and Saints: One of *St. Thomas de Cantelupe* Bishop of *Hereford*, in that Cathedral. These now remain. There was one in the Church of *Burton-Coggles, Lincolnshire*; and of *Heckington* in the same County; and innumerable others, destroy'd at the Dissolution of Monasteries.

The Shrine before us, from the Manner of drawing, and Workmanship, I conclude to be of *Saxon* Antiquity, and that very high; now near 900 Years old. I think it gives us the Story of the Murder of the Abbot there, and his Monks, perpetrated by the barbarous *Danes*, in the Year 870.

Sept. 25. that Year, they rush'd into the Church of *Croyland*, whilst the Religious were at divine Service. *Ingulphus*, Abbot of the Place, in his History, gives us this Account. Lord *Theodore* was then Abbot of *Croyland*; who at that time pontifically officiated at the high Altar, expecting the Barbarians. King *Osketyl* cut off his Head upon the Altar. *Verus Martyr et Christi hostia immolatur,*
says



The Backside of the Shrine. This elegant Antiquity is in Possession of
S^r John Cotton Baronet. 1748.

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says our Author, *Ministri circumstantes omnes capitibus detruncati*: " Thus fell the true Martyr and " Lamb of Christ, as a Sacrifice on the Altar. All " the assistant Ministers were beheaded likewise," says he.

The two on our Shrine are Frier *Elfget* the Deacon, and Frier *Savin* the Subdeacon.

Some Days after, when the Monks that fled returned, they found the Body of the venerable Abbot *Theodore* beheaded at the Altar.

Above is represented his Successor Abbot *Godric*, with the Ministers about him, putting the deceased Abbot into his Shroud; whilst Angels are carrying his Soul up to Heaven.

I suppose some Part of this Martyr might be obtained, and kept in this Shrine.

I observe the famous old sepulchral Stone in *Peterborough* Minster-Yard, is exactly of the same Shape as our Shrine. It was set up over the Grave of the Abbot and Monks murder'd by the same *Danes*, the Day after those of *Croyland-Abbey* suffer'd, *Sept. 26*. It is carv'd on the Sides with the Images of our Saviour and the Apostles. It is now remov'd into the Library.

Nov. 23. 1748.

W. Stukely.

- II. *An Eclipse of the Sun, July 14. 1748. observed by the Right Honourable James Earl of Morton, Mr. le Monnier, Royal Astronomer and Member of the Royal Academy of Sciences at Paris, and Mr. Ja. Short, Fellows of the Royal Society.*

Read Dec. 8. 1748. **T**HESE Observations were made at *Aberdour* Castle, belonging to the said Earl, whose Latitude is $56^{\circ} 4' N$.

Mr. *le Monnier* having come over from *France* to go to *Scotland*, to observe the annular Eclipse of the Sun, *July 14. 1748*. I was desirous to contribute all that lay in my Power to assist him, and therefore resolved to go to *Scotland* with the Right Honourable the Earl of *Morton*, who was so good as to permit us the Honour of accompanying him.

We arrived at *Edinburgh* *July 4.* and immediately went to the College, to enquire what Preparations were made there, in consequence of Letters we had wrote before we left *London*; when Mr. *Alexander Monro*, Professor of Anatomy, informed us, that, upon Receipt of ours, he had wrote circular Letters to all his Friends in different Parts of the Country, to prepare, in the best manner they could, for the most exact Observation of this Eclipse.

We found that the meridian Mark, which had been settled from Observations, by the late worthy Mr. *Mac Laurin*, was lost, by the taking down of a Chimney, upon which it was fixed; and Mr.
Matthew

Matthew Stewart, the present Professor, having no proper Instruments, had not as yet re-established it; which we hoped to do by an Instrument, which we every Day expected from *London*; and Mr. *Stewart* having promised to make the best Observation he could, we resolved to set out for *Aberdour*, a Seat of the Earl of *Morton's*, which he readily offered to us, and did us the Honour to accompany us thither himself, having the same Desire and Curiosity to do whatever lay in his Power to contribute to an exact Observation.

Aberdour is about 10 Miles almost N. W. of *Edinburgh*. We chose this Place; as being, by the Computations of this Eclipse, at or very near the Southern Limit of the *Annulus*.

In the Castle of *Aberdour*, Lat. $56^{\circ} 41' N.$ and $25''$ of Time West of the College of *Edinburgh*, we set up a Clock, *July 9.* and the Weather being cloudy, and our Equal-Altitude Instrument and *Transit* not being yet arrived, we on the 11th made use of an Equatorial Telescope of my Lord *Morton's*; to find corresponding Altitudes of the Sun, and at the same time put up a *Gnomon* of 15 Feet high.

Being uneasy that our Instruments were not come to Hand, and resolving to have a Communication with the College of *Edinburgh*, where they had a *Transit* Instrument; my Lord *Morton* proposed that two Cannon should be fired from the Castle of *Edinburgh*, one precisely at 12 o' Clock, and the other at 5' after 12' on the Day of the Eclipse; and the different Observers in different Parts of the Country to be advertised of this, and to mark down the precise Time of seeing the Flash, or hearing the
Sound

Sound of the Cannon; so that, after having made a geographical Map of these different Parts of the Country, and having found the exact Meridian of one Place, we should be enabled to settle the Times of all the rest by the Difference of Meridians found by this Map. This was settled and agreed to on the 12th, and an Express sent over to *Edinburgh* with a Letter from my Lord *Morton* to the Lord Justice Clerk, to desire this Favour of General *Bland*, who very readily granted it.

The 13th being a clear Day, we took equal Altitudes with the Equatorial Telescope, and found our Clock gained 1' 46'' in two Days, and that the Sun passed the Meridian at 12^h 7' 6'' by the Clock.

July 14th was an exceeding bad Morning both for Wind and Rain; but about 8^h in the Morning, the Clouds dispersed, and we had a very clear Sun.

In order to observe the Eclipse, my Lord *Morton* made use of a reflecting Telescope, 12 Inches focal Length, magnifying about 40 times. I made use of a reflecting Telescope 4 Feet *Focus*, magnifying about 120 times; both belonging to my Lord *Morton*. Mr. *le Monnier* made use of a refracting Telescope, about 9 Feet *Focus*, which he brought with him from *France*, armed with a Micrometer, made after the Method of Mr. *George Graham*, by the late Mr. *Sisson* at *London*.

Mr. *le Monnier* took his Station in the Garden, under the Window of the Room where the Clock was placed; my Lord *Morton* was in the Room next that where the Clock stood; and I was at the Window next the Clock.

Clock.

Clock.	True Time.	
8 55 0	8 47 5	The Eclipse not yet begun. Clouds come on.
8 59 13	8 51 18	Beginning of the Eclipse, found by the following Chord.
9 0 42	8 52 47	First View of the Eclipse, then considerably advanced.
9 2 30	8 54 35	Measured the Chord of the Part eclipsed; which was found equal to the Field of the great Reflector.
10 6 10	9 58 12	The illuminate Part of the Sun, measured by the Mi- crometer, and found = 7'
10 45 0	10 37 0	37'' $\frac{1}{2}$. Again measured, and found = 7' 37'' $\frac{1}{2}$. My Lord <i>Morton</i> judged the Middle of the Eclipse, or nearest Approach to an <i>An- nulus</i> , at 10 ^h 17' 54'' ap- parent Time.
11 52 43	11 44 40	The same Phase or Chord observed as at the Begin- ning, and measured both in the Telescope, as at first, and by the Micrometer, and found = 8' 25'' of a great Circle, as verified by a Base after the Eclipse was over, which gives the End as exact as the Beginning.

Clock.	True Time.	
h ' "	h ' "	
11 56 21	11 48 18	End of the Eclipse by the preceding Chord.

Mr. *le Monnier* measur'd with the Micrometer the apparent equatorial Diameter of the Moon, when she was upon the Sun; which he found = $29' 47'' \frac{1}{2}$. He measured also the apparent vertical Diameter of the Sun at Noon; which he found = $31' 40''$.

The Micrometer, with which he measured these Diameters, was afterwards verified, by a Base of 2570 Feet, and two Marks, placed at right Angles to its Extremity; at the Distance of 22 Feet from one another.

The Flash of the first Cannon fired from the Castle was seen at $12^h 3' 4''$ by the Clock; and the Flash of the second Cannon also by the Clock at $12^h 8' 4''$. The Eclipse was so nearly annular, that, at the nearest Approach, the Cusps seemed to want about $\frac{1}{7}$ of the Moon's Circumference to be joined; yet a brown Light was plainly observed, both by my Lord *Morton* and myself, to proceed or stretch along the Circumference of the Moon, from each of the Cusps, about $\frac{1}{3}$ of the whole Distance of the Cusps from each Cusp; and there remained about $\frac{1}{3}$ of the whole Distance of the Cusps not enlightened by this brown Light; so that we were for some time in Suspense whether or not we were to have the Eclipse annular with us. I observed, at the Extremity of this brown Light, which came from the Western Cusp, a larger Quantity of Light, than in any other Place, which at first surprized me; but afterwards

afterwards I imagined it must have proceeded from some Cavity or Valley made by two adjoining Mountains on the Edge or Limb of the Moon: I had often formerly observed Mountains on the Circumference of the Moon, more or less every-where round it, but never saw them so plain as during the Time of this Eclipse; for we had the Air exceeding clear, and free of all Agitation, notwithstanding it blew a perfect Hurricane of Wind, which began about the Middle of the Eclipse; and I remember, in the annular Eclipse of the Sun in the Year 1737. it did the same. The mountainous Inequalities on the Southern Limb of the Moon were particularly remarkable; in some Parts Mountains and Valleys alternately; others extended a considerable Way along the Circumference, and ended almost perpendicularly like a Precipice. My Lord *Morton* was able to see them very easily thro' his small Reflector.

A little after the Middle of the Eclipse, some Clouds, that seemed stationary below the Sun, appeared tinged on their upper Extremities with all the Colours of the Rainbow.

During the greatest Darknests, some People, who were in the Garden adjoining to the Castle, saw a Star to the East of the Sun; which, when they afterwards told us, and pointed to the Place where they had seen it, we found, must have been the Planet *Venus*. This Star, we were afterwards told, was seen also at *Edinburgh*, and other Places, by a great Number of People; but I did not hear of any other Stars being seen. The Darknests was not great, but the Sky appeared of a faint languid Colour. What is

H h h h

pretty

pretty remarkable, is, Mr. *le Monnier* assured us, that when he looked at the Sun with his naked Eyes during the Middle of the Eclipse, he could observe nothing upon the Sun, but saw the Sun full, tho' faint in his Light. This, I am apt to imagine, may be owing to his being short-sighted.

I observed also, about the Middle of the Eclipse, a remarkable large Spot of Light, of an irregular Figure, and of a considerable Brightness, about 7' or 8' within the Limb of the Moon next the Western Cusp. I thought I lost this Light several times; but whether this was owing to my shutting my Eyes, in order to relieve them, or not, I cannot tell. I am told, that the Rev. Mr. *Irwin* at *Elgin* observed the same. When I first perceived it, I called to my Lord *Morton*, who was in the next Room, but he could not see it.

Before the Eclipse began, and during the whole Time of the Eclipse, the Air, as I said before, being exceeding clear, I saw thro' the four Foot Reflector, the Surface of the Sun cover'd with something which I had never observed before; it seemed to be all irregularly overspread with Light, and a faint Shade, especially towards his equatorial Diameter. This Appearance was so odd, that it is difficult to describe it, so as to give an adequate Idea of what I saw; but if I may be allowed the Expression, it seemed as it were curdled with a bright and more dusky Light or Colour. This Appearance was permanent, and regularly the same; and if in any degree seen before, may have given Rise to *Faculae* having been seen in the Sun; but to me the whole Sun's Body seemed to be more or less cover'd with it.

I looked

I looked with all the Attention possible, to see if I could observe the Body or Limb of the Moon before she touched the Sun, and also after she left it, and was intirely off the Sun, but could see nothing at all of any such Appearance. I mention it to satisfy Mr. *De Lisle*, who publicly desired this might be attended to.

The Barometer had been falling for several Days before the Eclipse; and even that Morning; when it was at 29.2 Inches. But during the Eclipse it began to rise.

				Divisions.
<i>July 11.</i> at 8 ^{<u>a</u>} in the Morning the Thermometer				
stood at	h	-	'	54
at 12	0	or Noon	at	56
at 4	0	<i>p. m.</i>	at	60
<i>July 12.</i> at 11	0	<i>a. m.</i>	it stood at	57
at 12	0	or Noon,	at	58
<i>July 13.</i> at 8	30	<i>a. m.</i>	it stood at	55 $\frac{3}{4}$
at 1	0	<i>p. m.</i>	at	57 $\frac{1}{2}$
<i>July 14.</i> at 8	0	<i>a. m.</i>	at	56
at 8	53	at		57
at 9	7	at		57 $\frac{1}{2}$
at 9	20	at		57 $\frac{1}{4}$
at 10	8	at		57
at 10	26	at		56 $\frac{1}{4}$

All these Observations of the Thermometer were taken when it stood in the Shade; and the Times are by the Clock. Immediately after the Middle of the Eclipse, the Thermometer, when exposed to the Sun for the Space of 10^o of Time, rose only half a Division.

H h h h 2 Ther.

Thermometer still exposed to the Sun,

at 10 ^h 46' 00", stood at	-	-	-	58 $\frac{1}{2}$
at 10 51 30 at	-	-	-	62
at 10 57 30 at	-	-	-	63 $\frac{1}{2}$
at 11 4 00 at	-	-	-	66
at 11 10 00 at	-	-	-	70
at 11 34 00 at	-	-	-	75 $\frac{1}{4}$

Thermometer replaced in the Shade after this last Observation,

at 12 ^h 54' stood at	-	-	-	60 $\frac{1}{2}$
at 1 28 at	-	-	-	61 $\frac{1}{4}$
at 5 50 at	-	-	-	59
at 7 30 at	-	-	-	58 $\frac{1}{2}$
July 15. Thermometer at 8 ^h a. m. stood at	-	-	-	56
at 9 at	-	-	-	57
at 10 at	-	-	-	60

These Observations were made with a Thermometer of *Fahrenheit's* Scale, the Divisions of which were very sensible. We did not at all perceive or feel any greater Degree of Cold, during the Eclipse, than we felt before it began.

The Weather being very bad at *Edinburgh*, Mr. *Matthew Stewart*, the Professor of Mathematics, could make no Observations of the Eclipse; he only saw the End at 11^h 50' 34" true Time; and even then the Sun was somewhat cloudy: He took however the Sun's *Transit* over the Meridian (as then supposed) at 12^h 7' 42" by his Clock, and heard the second Cannon fired from the Castle at 12^h 4' 48" by the Clock. We afterwards, in a few Days, examined his meridian Mark with a very exact equal Altitude Instrument by
three

three several correspondent Observations ; and found his Mark $3' 22''$ of Time to the West of the true Meridian. The College is about 2500 Feet distant from the Castle Eastward.

The Rev. Mr. *Bryce*, at *Aldiston*, about 6 Miles to the West of *Edinburgh*, Lat. $55^{\circ} 55\frac{1}{2}'$ N. observed with a reflecting Telescope, 9 Inches *Focus*,

The Beginning of the Eclipse at	8 52 30
Upper Horn or Cusp vertical, at	9 5 0
Hitherto the Western Cusp lower than the Eastern.	
The two Cusps horizontal at	10 13 10
The Western Cusp ascends very fast at	10 14 10
The Western Cusp vertical at	10 16 15
The Cusp which was just now vertical, now becomes East, and about 30° from the Zenith to the East at	10 17 10
The Middle of the Eclipse as near as he could judge at	10 17 40
The lower Cusp at the <i>Nadir</i> , and very ragged and uneven, at	10 24 45
The same Cusp still in the same Position at	10 32 5
The same Cusp seems to begin to move towards the West at	10 43 35
The Motion of this Cusp scarce sensible at	10 55 45
The other Cusp Middle between the <i>Zenith</i> and the <i>Nadir</i> towards the East at	11 0 25
End of the Eclipse, the Sun being quite clear at	11 48 40

I shall set down the following Observations of this Eclipse just as they came to my Hand when in *Scotland*, without making any other Remark, than that, from the Disagreement among themselves, they do not all of them seem to have been made with due Accuracy and Attention; for want, I suppose, of sufficient Practice in this kind of Observations.

William Crow Esquire, at his House of *Netherbyres* near *Haymouth*, Lat. $55^{\circ} 51' N.$ says,

The Eclipse began at	8 55 0
Half of the Sun eclipsed at	9 50 0
Middle of the Eclipse, $\frac{1}{2}$ of the Sun's Limb cover'd by the Moon at	10 25 0
End of the Eclipse at	11 55 0

Mr. John Mair, at *Air*, Lat. $55^{\circ} 30' N.$ says, the Eclipse began at $8^h 45'$; but that, by reason of Clouds, he could make no other particular Observation; only that, by a View he had of the Sun some little Time before the End, he thinks the End of the Eclipse might be about $11^h 48'$.

Mr. Mark, Teacher of the Mathematics at *Dundee*, Lat. $56^{\circ} 25' N.$ observed,

The Beginning of the annular Appearance at	10 16 44
End of the annular Appearance at	10 23 8

He says, the best Observations make the *Annulus* a small Matter narrower on the upper than lower Side; by which it appears the Centre of the Eclipse was to the Northward of *Dundee*.

Mr.

Mr. *John Stewart*, Professor of Mathematics at *Aberdeen*, writes, that, by an Observation made at *Monrofs*, Lat. $56^{\circ} 41'$,

	"	"
The annular Appearance began at	10	20 0
<i>Annulus</i> ended at	10	24 30
End of the Eclipse at	11	52 45

"And that, by an Observation made at a Place about 18 Miles S. W. of *Aberdeen*,

	"	"
The Eclipse began at	8	52 0
Middle at	10	21 0
End at	11	52 0

"And that at *Aberdeen*, Lat. $57^{\circ} 11' N$.

	"	"
The Eclipse began at	8	55 33
Middle of the Eclipse, and annular Ap- pearance, as near as he could judge, at }	10	23 3
End of the annular Appearance at	10	24 48

He writes also, that he received an Account from Mr. *Reid*, Minister at *New Macchar*, about 7 Miles N. W. of *Aberdeen*, who observed

The Beginning of the annular Appearance at	10	18 28
And the End of the Eclipse at	11	49 3

Mr. *Stewart* says, that, by comparing his Observation at *Aberdeen* with this of Mr. *Reid's*, he apprehends he is in a Mistake as to his judging of the Middle of the Eclipse, and annular Appearance; and reckons, that the annular Appearance began at *Aberdeen* at $10^h 19'$, and ended as above. By which the total Duration of the *Annulus* was $5' 48''$; and the End of the Eclipse at *Aberdeen* was at $11^h 49' 33''$.

The

The Rev. Mr. *Irwin*, at *Elgin*, Lat. $57^{\circ} 34'$, says, the Eastern Limb of the Moon touched or entered on the Western Limb of the Sun at $8^h 57'$; tho' he suspects it began a little sooner (another having taken the Telescope out of his Hand); for when he looked, the Moon was a little advanced on the Disc of the Sun about 30° from the *Zenith* of the Sun towards the West.

The Eastern Cusp in the *Zenith* of the Sun at $9 \quad 6 \quad 10$
 Eastern Limb of the Moon reached the } $9 \quad 39 \quad 0$
 Centre of the Sun at

The *Annulus* began about 30° from
 the *Zenith* of the Sun Westward at } $10 \quad 20 \quad 0$

The *Annulus* appeared most perfect at $10 \quad 22 \quad 45$

Tho', as nearly as he could discern, he thought it a little narrower on the South-west Limb of the Sun, than it was on the opposite Side. From hence it should appear, that the Centre of the Eclipse was to the Southward of *Elgin*.

The *Annulus* was observed to break on the South-east Limb of the Sun, about 30° from the *Nadir*, at $10^h 25' 30''$.

Before the joining of the Cusps of the Sun, as also at the breaking of the *Annulus*, he says, he observed a quick tremulous Motion, and several irregular bright Spots between the Cusps, which disappeared in a few Moments; and he thought the Moon's Body passed quicker about the Time of the *Annulus* (especially as it was forming), than at any other Time during the Eclipse.

Before the Western Limb of the Moon reached the Centre of the Sun's Disc, the Sun was hid under

a Cloud, and continued so, till within some little Time of the End of the Eclipse, which happened at 11^h 50'.

There was no Cloud all the Time of the Formation of the *Annulus*, or the Duration of it; and he thinks he is pretty right, as to the Time of its Continuance; for both the Formation and Breaking were very sensibly to be observed, and passed in a Moment; affording a very pleasant Sight, by the irregular tremulous Spots of the Sun.

He says, the Darkness, during the *Annulus*, was not so great as a little before and after; and, when greatest, was only somewhat dusky, but observable. Some saw a Star to the East of the Sun; but he saw it not, nor any present with him. He was told of it after his Observation was over.

He says, that, by an Observation taken of the Sun that Day at Noon, he found that his Clock was somewhat less than a Minute faster than the Sun. He says also, that he observed this Eclipse with a Telescope 3 Feet long, and that he had a very good Burning-glass; but that it had little Force, during the *Annulus*, and some short time before and after.

Mr. *Duncan Frazer* writes to Mr. *Monro*, Professor of Anatomy at *Edinburgh*, that he went to the House of *Culloden*, Lat. $57^{\circ} 29'$ N. on purpose to observe the Eclipse; it having been said, that the Centre of the Eclipse would pass there; and after having adjusted his Clock by the Regulator-Clock of a Watch-maker at *Inverness*, he observed the Eclipse with a Telescope five Feet long, and found

The Beginning precisely at	8 37 36
Beginning of the <i>Annulus</i> at	10 0 10
End of the <i>Annulus</i> at	10 5 10
End of the Eclipse at	11 29 30

By comparing his Observation with that sent him by Mr. *Irwine* at *Elgin*, he imagines his Clock was not set to true Time, since there is so great a Difference, and more than the Difference of Longitude between the two Places will allow; it being no more than 26 computed Miles, and nearly in the same Parallel of Latitude.

Mr. *Murdock Mackenzie* (who has for some Years past been making a Survey of the Islands of *Orkney*, and whose Abilities for such an Undertaking give us Hopes he will for the future free Navigators of a great many melancholy Disasters, which formerly happened in those Seas, thro' the Want of true Charts) made the following Observation at *Kirkwall* in the Island of *Pomona* in *Orkney*, the Latitude of which is $58^{\circ} 58'$ N.

Beginning of the Eclipse about	8 40
End of the Eclipse about	11 37

He says, that, by reason of Clouds, he could not be perfectly exact, as to the precise Time of Beginning or Ending; but adds, that the Beginning cannot be more than 4' wrong, nor the End more than 2'. He says, he is sure he did not see it annular, but that there remained about $\frac{1}{4}$ or $\frac{1}{2}$ of the Sun's Circumference intercepted at the Middle of the Eclipse.

P. S.

P. S. It having been an Opinion pretty generally received, that the darker Parts of the Moon's Surface are Water, I take this Opportunity to remark, that though those less lucid Spaces are for the most part, to Appearance, evenly extended Surfaces, when Telescopes of small magnifying Powers are made use of, yet, when they are examined with larger Magnifiers, it is easy to discern on them many Protuberances in a longitudinal Direction; and that these Risings are really elevated above the common plane Surface, is past all Question, from their projecting Shadows, always opposite to the Sun: Moreover they are of the very same Colour as the Plane they arise from; of the like smooth Surfaces, without any sensible Asperities; and invariably the same, under the like Positions of the Sun to the Moon, at least as far as I have been able to discover in 12 or 15 Years frequent Observations of them.

Ja. Short.

III. *A Letter from Mr. David Erskin Baker to Martin Folkes Esq; Pr. R. S. containing Considerations on two extraordinary Belemnitæ.*

S I R,

Read Nov. 24. 1748. VARIOUS have been the Opinions of Authors concerning the Origin of the *Belemnitæ*, and as various the Systems and Hypotheses advanced by them in Support of their Opinions; some having imagined them vegetable Productions; others have taken them for the different Parts of Animals, as Teeth, Horns, Bones, &c. in which even these again have differed, as to the referring them to land or marine Animals; and they have been by others supposed of mineral Origin, or *Lapides sui generis*. What they really are, will, I doubt, be still very difficult to determine; but, as one principal Objection to their being originally marine Bodies (which Supposition seems to carry the greatest Colour of Probability) has been, that no marine Bodies have been found adhering to them, that Objection will be obviated by no less than two Specimens, from the same Place, of *Belemnitæ*, whereto undoubted marine Substances are found firmly affixed; by which Instances, as some further Light may be thrown on this Subject, that Consideration will, I hope, stand as an Excuse for my troubling you with this Paper.

These curious Fossils, which, together with the Drawings of them, I humbly submit to your Examination,

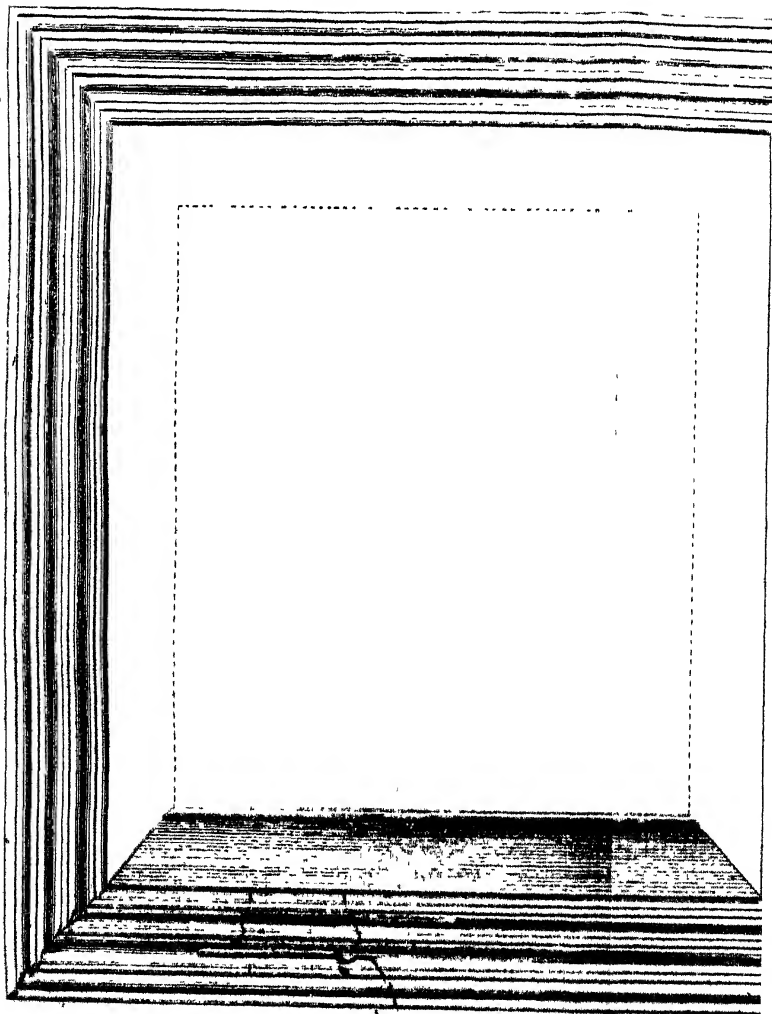


Fig. 4. p. 609.

Fig 1
p. 599.



Fig 5



p 612.



Fig. 2.
p. 599.



Fig. 3. 6
p. 599.

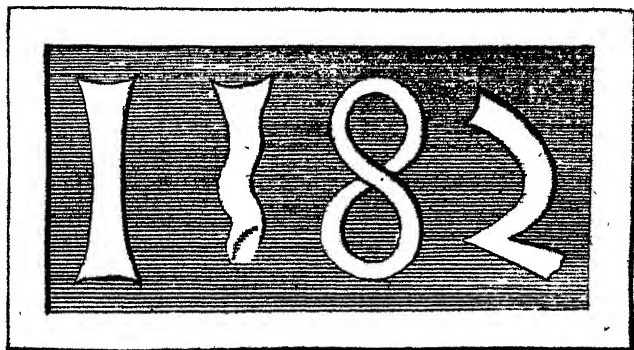


Fig. 6. p. 613.

J. Mynde sc.

mination, were found in a Chalk-Pit in *Norfolk*, from whence they were sent not long since to my Father Mr. *Henry Baker* F. R. S.

See TAB. IV.

Fig. 1. Is a *Belemnites*, whose *Apex* is perfect; the conic Cavity, and the longitudinal Seam, evidently distinguishable; which, as well as the Contexture of the Substance whereof it is composed, shew it to be a true *Belemnites*; but on its Surface are placed, in their natural Condition, by which I mean not at all seemingly petrify'd, or otherwise alter'd, two of those *Vermiculi* that are so frequently found sticking to Oysters, Scallops, and many other kinds of Shells, when taken out of the Sea.

Fig. 2. A *Frustum* of another *Belemnites*, the *Apex* whereof is broken, but the conic Cavity is still remaining, and shewn at *a*. To this *Belemnites* adheres a Shell of the Oyster-kind, which is fasten'd thereto so strongly, that they are not to be separated without breaking: Which Shell, as well as the before-mention'd *Vermiculi*, seems not altered in its Substance, but appears like a recent one, of which many are to be met with in the Cabinets of the Curious.

Fig. 3. Shews the other Side of the said Shell, wherein the *Cardo* or Hinge at *b* is plainly discernible; at *c* appears the broken End of the *Belemnites*, where the radiated Contexture (well known to belong to their Bodies) is represented, as also the longitudinal Seam at *d*.

As

As these Specimens are undeniable Proofs of marine Bodies adhering to *Belemnitæ*, several of the Curious who have seen them, are of Opinion, that they tend likewise to prove the *Belemnitæ* to be marine Productions. It may probably be objected, that these Shells might have been brought and deposited near the *Belemnitæ* whereto they are affixed, by whatever mighty Change it came to pass that Productions of the Sea are discover'd in most Countries at great Depths in the Earth, and in the Bowels of Mountains at great Distances from the Sea (even supposing the *Belemnitæ* to be *Lapides sui generis*, and produced in the Earth) and that these Shells might be cemented to them afterwards by some mineral, stony, or other Matter. But the following Observations will render this improbable; for,

1. The *Vermiculi* of *Fig. 1.* are not any Species of the *Tubuli marini*, found sometimes recent, and sometimes fossil, detach'd intirely from every other Body; but are of that sort, which is perhaps never seen separate, or in any other Manner, when recent, than attach'd and fasten'd to other Shells or Stones; and they are placed on this *Belemnites* exactly in the same Manner as they are commonly found on other marine Bodies; viz. lying on their broadest Side, with their Ridge upwards, and glued as it were thereto by a shelly Substance.

2. In *Fig. 3.* at *e*, is plainly to be distinguished, that the Shell has been fashion'd thus by the convex Surface of the *Belemnites*, in the same manner as these Shells commonly receive a Form from whatever Substance they adhere to; which plainly implies, that this Shell was fasten'd to the *Belemnites* when
itself

itself was very small, and in a growing State; and that the Shell in its Growth was formed according to the Figure of the Body on which it was affixed: But such Growth could not possibly have proceeded any-where but in the Sea; and therefore these two Bodies must necessarily have been in the Sea at one and the same time.

There is now but one Way more, whereby these Shells (supposing the *Belemnite* to be Stones *sui generis*) could possibly become affixed to them; which is, that the *Belemnite* might have been by some Accident thrown on the Sea-shore; and that there the Shells might fasten themselves to them, as well as to any other Stone. But as this must imply some former Convulsion in Nature, whereby they were cast out of their natural Beds upon the Sea-shore; and again a second Convulsion to carry them to the Chalk-pit where they were found; so far-fetch'd an Objection will, I believe, carry but little Weight.

To conclude, I submit to your Opinion, whether the Sides of the conic Cavity, whereto the Oyster-shell is affix'd, has most the Appearance of a Stone or of a Shell; and remain, with the utmost Respect,

S I R,

Strand, Dec. 15.

1748.

Your most obedient and

obliged humble Servant,

D. E. Baker.

IV. *A Letter from Benj. Cooke F. R. S. to Peter Collinson F. R. S. concerning a mixed Breed of Apples, from the Mixture of the Farina.*

Dear Cousin,

Newport, Dec. 4. 1748.

Read Dec. 22. 1748. **I** SENT you last Year a Specimen of the Effect of the *Farina* of a rough-coat Apple striking on the Flower of a smooth-coat; I have now sent an Example of the *Farina* of the latter changing the former into its own Dress and Likeness.

The Situation of the Ruffeting was such, that he was surrounded by Winter Pippins, Pearmains, and such-like; and we put the Master-Fruit together with several of the Changelings, as they grew on the same Branches mixed together.

This Instance will shew what Alterations may be expected in cognate Species; and I should have given an Example of a kind of Antipathy betwixt the Pear and the Apple in like Circumstances, but was disappointed. I am,

Dear Cousin,

Yours, most obliged and affectionate,

B. Cooke.

V. *A DESCRIPTION of the town of SILCHESTER in its present state. With a short ACCOUNT of an antient DATE in Arabian figures at Walling near Aldermarston in Berkshire.*
By John Ward, F. R. S. and P. R. G.

Read Dec. 22.
 1748.

IN a former paper, which I had the honour to lay before this Society (1), I attempted to explain a Roman inscription cut in a stone, then lately found at *Silchester* in *Hampshire*; by which it not only appeared, that this town was the antient *Vindomis*, but likewise that it was situated within the limits of the *Segontiaci*: as to both which circumstances our best antiquaries have been at an uncertainty, and differed in their sentiments concerning them. I took notice likewise at the same time, that the traces of the antient town are yet, as I had been informed, often visible in the summer; and that the ruins of an amphitheatre still remain without the wall. But being since in that country, I had an opportunity of visiting the place myself, and getting a more perfect account of it than I expected, by the assistance of two persons in the neighbourhood, the late Mr. *John Wright* junior, and Mr. *John Stair* junior, who were both well acquainted with it, and accompanied me thither.

The

(1) *Phil. Transf.* Num. 474.
 Kkkk

The former, who was an experienced surveyor, measured the whole circuit of the wall, with the height of it in several places, as also the dimensions of the amphitheatre, while we were on the spot. And the other traced out the several streets, and other parts of the town, to a considerable exactness. But as only a rough draught of the plan could then be taken, I left it with them to revise, and transmit to me an accurate copy; which has been since done, and now accompanies this description (a):

The circuit of the wall on the outside, as therein given by the scale, contains near one *English* mile and a half; and the several parcels of land contained within it amount together to an hundred acres, or upwards. Indeed *Leland* says, that the compass of the wall is *about two miles, and conteyneth 80 acres* (1). And *Camden* says the same, except that he calls them *Italian miles* (2). But neither of them acquaint us, from whom they had their measurement. The wall consists of nine sides, but very unequal; which might perhaps be occasioned by the different situation of the ground, which in some parts is uneven.

The materials, that compose the wall, are large flints, and rough stones of different sorts, cemented together with very strong mortar. And as to the manner of building it, the foundation is generally made of a row or two of stones laid flatwise; and
over

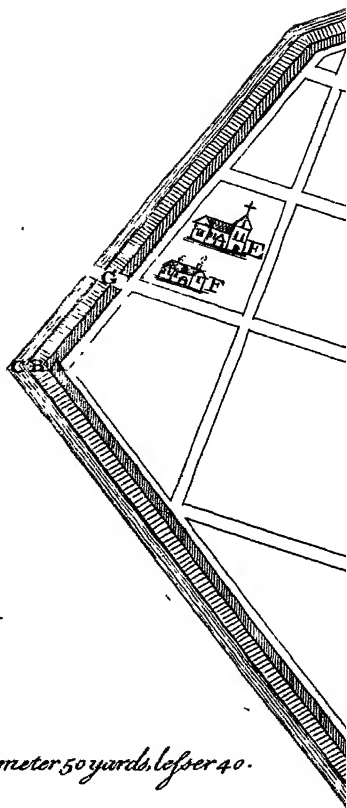
(1) *Itinerary*, vol. vi. p. 48, edit. 1744.

(2) *Britann.* p. 196, edit. 1607.

(a) See TAB. III.

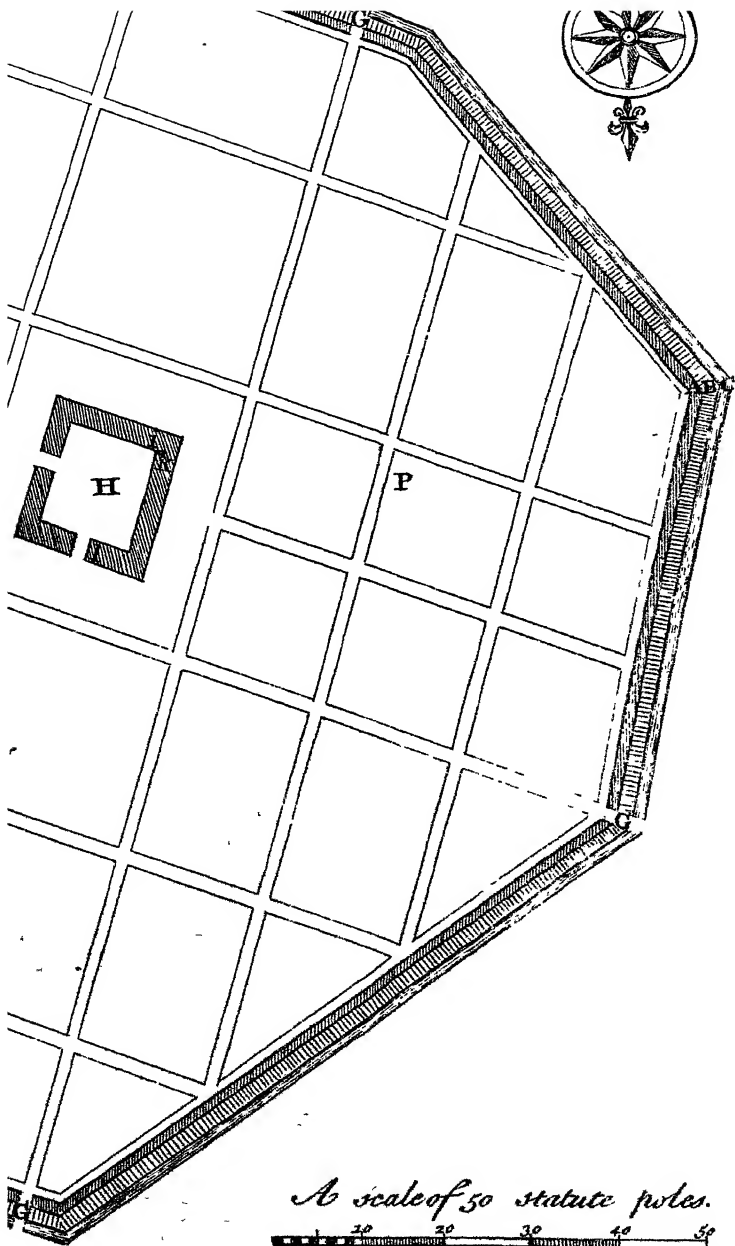
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A PLAN of
the ancient city of
SILCHESTER
in *Hampshire*,
taken by
JOHN WRIGHT
Surveyor,
MDC CXLV.

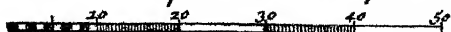


The Explanation.

- A.** *The Wall.*
- B.** *The Vallum.*
- C.** *The Ditch.*
- D.** *The Amphitheatre, greater diameter 50 yards, lesser 40.*
- E.** *The Church.*
- F.** *The Farmhouse.*
- G.** *The four City gates, which terminate the two chief streets.*
- H.** *The Forum.*
- ik.** *A Temple.*
- 1.** *The Roman inscription.*
- Mn.** *A Fountain.*
- O.** *Onion hole.*
- P.** *Silver hill.*



A scale of 50 statute poles.



J. Myrds se

over them four or five rows of flints; then usually a double row of stones, sometimes three rows, and at other times one only, laid in the same position; over these a like number of rows of flints, as before; and so alternately upwards. And a little to the westward of the south gate are yet to be seen seven of these ranges of stone, with six of flint between them; where the height of the wall measured on the outside about eighteen feet. And about fifty yards eastward of the same gate are six ranges of stone, with five of flint between them; where a small part of the facing seems yet to be near intire. But there is no appearance either of copings, or battlements, on any part of the wall. Tho the ranges of stone in the front of the wall are placed horizontally, yet those within it often stand edgewise and somewhat obliquely, like the wall of *Severus* in the north of *England* (1). And at the south gate the thickness of the wall measured about five yards. From this account therefore it seems not improbable, that in the passage of *Leland*, given us by Mr. *Hearne* from *Stowe's transcript*, where it is said, *the wall without is in some place 6 or 7 fote highe* (2), for the numbers 6 or 7 should be read 16 or 17.

The wall is not any where intirely demolished, except that two breaches have been made of late years on the north-west side, to open a passage for waggons.

(1) See *Horsley's Britann. Rom.* p. 123.

(2) *Ubi supra.*

waggon. And the ditch without the wall is in some places ten or twelve yards over, but in others at present not visible; where probably it may have been filled up by the earth thrown into it from the *vallum*, that encompassed the city between that and the wall, and which is yet in several places of a considerable height above the ditch. Each of these may be seen in the plan, (a) marked with the letters A, B, C. There is little appearance of the *vallum*, or military way, within the circuit of the wall, the ground being now more generally raised pretty near the top of the wall, on which grow many large oaks, and other timber trees. From the south gate towards Winchester has lain a military road, which when broken up appears to have been pitched with flints.

The amphitheatre stands without the wall, at the north-east corner, and distant from it upwards of an hundred yards; as appears in the plan, where it is marked by the letter D. Both the wall and seats, which are made in it, consist of a mixture of clay and gravel. The wall is about twenty yards thick at the bottom below the seats, and decreases gradually to the thickness of about four yards at the top. There are five ranges of seats above one another, at the distance of about six feet on the slope. It has two passages into it, one towards the town, and the other opposite to it. The diameter of the area is fifty yards by forty, and the area itself now serves for a pond to a farmer's yard. The design of this amphitheatre might possibly be for the baiting

ing of wild beasts, or other athletic diversions, agreeable to the customs of those times. Tho at present no appearance of a cavern, or any other place proper for the reception of such animals, is to be discover'd.

The area of the town within the walls contains at present only corn fields, except a small quantity of meadow land, with an antient church, and farm house, near the east gate, both which are marked in the plan with the letters E and F. The method taken by Mr. *Stair*, in order to discover where the streets formerly lay, was by observing for several years before harvest those places, in which the corn was stunted, and did not flourish as in other parts. These were very easily distinguished in a dry summer, and run in strait lines crossing one another, as they are drawn in the plan. Moreover, by spitting the ground, and often digging it up, he found a great deal of rubbish; with the plain ruins and foundations of houses on each side of these tracts. Whereas in the middle of the squares nothing of that nature appeared, and the corn usually flourished very well. The ploughmen also confirmed the same, who found the earth harder, and more difficult to be turned up, in these tracts and near them, than elsewhere. And it is further observable, that two of these streets, which seemed rather wider than the rest, lead to the four gates of the city, one of them running in a direct line from the north to the south gate, and the other from the east to the west, which latter measured at least eight yards across. The four gates are marked with the letter G in the plan.

By digging likewise in different places Mr. *Stair* at length discovered the ruins of a number of buildings

ings, in the form of a long square, which in the plan is marked with the letter H. The foundations of some of these buildings were still pretty intire, and the depth of them from wall to wall was found to be about twenty seven feet, and the breadth about sixteen, which it is not improbable may be the remains of the antient *forum*. But between the letters *i* and *k* there appeared the foundation of some larger structure, consisting of free stone three feet in thickness. And at *k* there seemed to be the pedestal or foundation of an altar, by the great quantity of ashes and wood coals burnt, that lay round about it. What remained was about three feet in hight, four in length, and three in breadth. It consisted of large *Roman* bricks, one of which dug up intire, and communicated to me by Dr. Collet, is seventeen inches and a half long, twelve and a half broad, and two and a half thick; which accompanies this paper.

At the letter *l* was found the stone with the inscription upon it mentioned above. And upon further search Mr. Stair has since dug up within two feet of the same place, and about four feet under ground, a square copper frame, composed of several mouldings, and its sides soldered together, three of which are yet intire, but part of the fourth is broken. This frame inclosed a border of the same metal, one side of which is still preserved. The weight of them together is forty seven pounds; but the thickness of the frame varies in different parts from one fourth of an inch to much less, and the border is more than one eighth of an inch thick. Each side of the
frame

frame at the outer edge is about thirty three inches long. And from the size of the inner edge of the border, each side whereof is twenty inches and a half in length, it is supposed that the stone, which contains the inscription, was at first placed behind it, and supported by it. And as that inscription was erected in honour of *Hercules*, it might originally belong to the stone building, whose foundation is yet visible, and which might have been a temple consecrated to that deity by the persons named in the inscription; as was conjectured in the former account given of it. A draught of this frame and border in their present state, taken by a scale of one sixth of the original (1), now in the possession of Dr. Mead, is prefixed to this discourse (a). There was likewise a considerable number of brass *Roman* coins found near the same place.

At the letter M in the plan was antiently a fountain, which at *n* discharged itself under the wall. Some of the stones, with which this fountain was inclosed, are still to be seen.

And at the letter o is a large breach under the wall, two yards in length, and two feet in high; tho

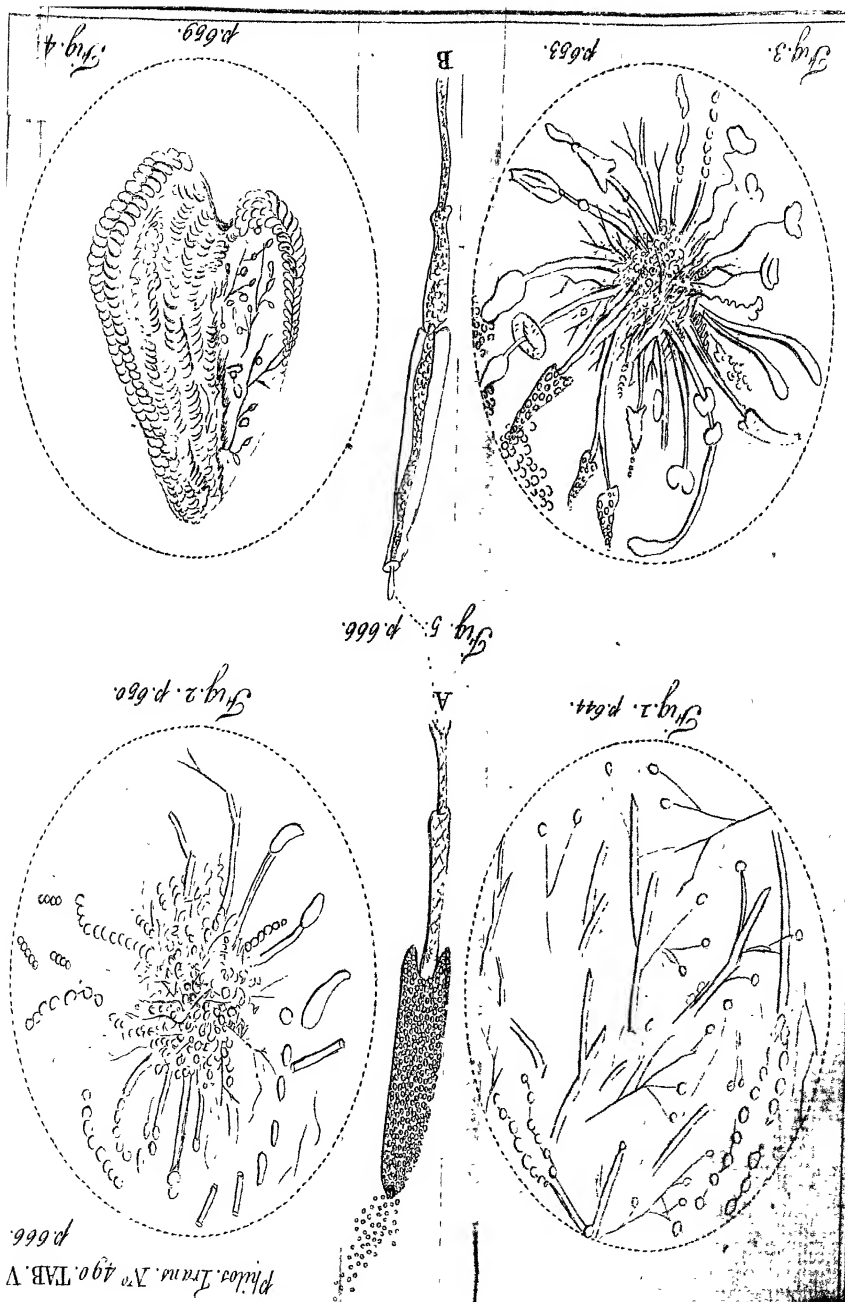
(1) It may be necessary to repeat here, what was remarked in the *Errata* of the *Phil. Trans.* N. 474, which were put under the *Contents* of N. 475. That whereas the draught of the *Roman* inscription mentioned in p. 201, is there said to have been taken by a scale of one fourth of the original; it was afterwards reduced by the engraver in the plate to near one sixth. And in that proportion it is to be compared with the draught of the frame here given, wherein it is supposed to have been antiently placed.

(a) See TAB. IV. Fig. 4.

tho in the memory of some persons yet living it was considerably higher. It is called *Onion hole*, from an imaginary giant of that name, who is said once to have inhabited this city; and from whom likewise the *Roman* coins found there have been called *Onion penies*. The breadth of the wall thro this hole, where it is pretty much broken, measured now but ten feet; and being upon high ground could not, I think, have been a common sewer, as some have imagined; but might originally perhaps have been designed for a private passage upon some necessary occasions.

The most valuable coin, which has been discovered in the ruins of this antient *Roman* town, is a gold one of *Allectus* in fine preservation, and very remarkable for a peculiar attribute of the deity on the reverse. The front side represents the head of *Allectus* crowned with laurel, round which is this legend, IMP C ALLECTVS P F AVG. On the reverse is placed the figure of *Apollo* with a radiated crown; his left hand, which holds a globe, has over it a whip; his right arm is raised in a forbidding posture, and supports a *chlamys*, which crossing his breast descends on both his sides; at his feet sit two captives, whose hands are tied behind them; and the legend round it is ORIENS AVG, with ML in the exergue (1). These several attributes may be found in

(1) Some very skilful antiquaries have thought, that those letters on the reverse of many coins of the lower emperours, which are put



in some or other of the imperial coins between the time of *Gordian* the younger and the *Constantines*, or later. But the singularity of the coin, I am now describing, is this; that in the figure of *Apollo* the eyes seem plainly to be covered by a fillet, which goes cross the forehead; the reason of which I cannot undertake to account for with certainty. But we are told by *Suetonius*, that among other reflections thrown upon *Augustus* for a secret entertainment made by him, at which the persons present were dressed in the habit of deities, and this at a time of great scarcity in *Rome*, *Acclamatum est postridie frumentum omne deos commedissee; et Cæsarem esse plane Apollinem, sed tortorem*. To which the historian adds, *Quo cognomine is deus quadam in parte urbis colebatur* (1); which being the place where criminals were punished, is thus described by *Martial*,

Cruenta pendent qua flagella tortorum (2).

It

put at the bottom, often denote the place, where those coins were struck. And therefore, as I meet with *ML* by themselves upon no others, but those of *Carausius* and *Allectus*, who both ruled in *Britain*; it seems not improbable, that they may stand for *moneta Londinensis*, or *Londini*, supplying *signata* or *incusa*. As upon some coins of *Constantinus Magnus*, who was first proclaimed emperor in *Britain*, we find *MSL* and *MLI*, which may also be so interpreted. Indeed the letters *MLS* and *MLP* occur upon the coins of some other emperours, where they have been read, and perhaps justly enough, *moneta Lugduni signata* and *percutsa*.

(1) In *vit. August. cap. 70*.

(2) *Lib. II. epigr. 17*.

It was not unusual for the antients to apply the attributes of one deity to another on particular occasions. From whence one might be led to interpret this representation of *Apollo*, or the Sun, with a whip, and a bandage over his eyes (the emblems of *Justice*) together with the two captives, as descriptive of the punishment denounced against all, who should attempt to oppose the government of *Allectus*. And as the *Roman* coins do generally contain on their reverse some devise relating to the times, in which they were made; it seems not improbable from hence, as also from the legend, *ORIENS AVGVSTI*, that this was struck upon *Allectus's* first assuming the purple, after he had murdered *Carausius*: since upon others of his coins we meet with *SPES AVGVSTI*, *VIRTVS AVGVSTI*, *PAX AVGVSTI*, *SALVS AVGVSTI*, and the like, as denoting the gradual success of his affairs afterwards. An impression of this curious coin, which is now in the *museum* of Dr. *Mead*, is likewise given here (a). The place, where it was found, is marked in the plan by the letter P (b); which of late years has gained the name of the *Silver hill*, because more silver coins have been found there, than in any other part of the city. And by the remaining ruins, which discover themselves upon turning up the ground, it is supposed, that some large building stood antiently upon that spot. But great numbers of coins in all metals, and of all sizes, have likewise been found in several other places; so that

(a) See TAB. IV. Fig. 5:

(b) See TAB. III.

that Mr. *Stair* is now possessed of several hundred, which have been all collected from this *Roman* settlement; among which are the emperours *Valentinian* and *Arcadius* in gold; with most of the imperial coins from *Augustus* to that time, either in silver or brass; many of which are exceedingly well preserved.

I SHALL only beg leave to subjoin here a brief account of an antient date in *Arabian* figures, which yet remains at *Walling* near *Aldermarston* in *Berkshire*. It is impressed in releivo upon a brick, near the top of a large and high chimney, on the outside of a farmhouse belonging to *William Wollascot* esquire of *Woolhampton* in that neighbourhood. This date had always hitherto been read 1182, the two first figures, as they are seen from the ground, having both the appearance of a one; with this difference only, that the second seems pretty much thicker than the first. And this led me upon viewing it in that situation to suspect it might be a three, like that in the *Cambridge* date, published in the *Philosophical Transactions*, N. 474. And accordingly having by the means of a long ladder an opportunity of going up to it, I found upon a near inspection, that it was really so, as I had apprehended. For the small curves in the second figure being filled up with moss gave it the appearance of a broad and strait line, when seen at a considerable distance. I took an exact draught of the whole, by first impressing a paper upon the brick, with the
several

several figures contained in it; and then delineating each of them carefully with a pencil. And the copy, which is here exhibited, is drawn by a scale of one third of the original (*a*). The house, where this date remains, is by tradition said to have belonged antiently to a knight templar; but however that might be, the date must have been placed there long afterwards; as that order of knights was destroyed on the seventh of *January*, in the year MCCCVII.

G. C. Dec. 13, 1748.

John Ward.

(*a*) See TAB: IV. Fig. 6.

VI. *A Summary*

A Summary of some late Observations upon the Generation, Composition, and Decomposition of Animal and Vegetable Substances; Communicated in a Letter to Martin Folkes Esq; President of the Royal Society, by Mr. Turbervill Needham, Fellow of the same Society.

Paris, Nov. 23, 1748, N. S.

S I R,

Read Dec. 15. 22.
1748.

§ 1. **T**HO' I think myself now almost sufficiently qualified, by the Multitude of Experiments I have already made upon animal and vegetable Substances, since the 16th, N. S. of last *March*, to lay down some certain Truths upon this Subject, and from them to advance, by Induction, farther than so short a Period of Time would allow me to proceed by special Experiments, yet I would have your learned *Royal Society* look upon this Paper as an imperfect Sketch only of what I hope to publish from the Journals I have by me in a few Months, if these two or three Sheets are so fortunate as to meet with their Approbation. I am sufficiently sensible how much I may hurt this little Performance, if I promise too much, and raise in this Matter higher Expectations from the Public than it may appear hereafter to deserve: It is at this time therefore particularly the more necessary, that I should be exceedingly cautious to advance no Proposition rashly; nothing, but what seems to flow naturally from Observation. But this Precaution, however

*

strict,

strict, will not exclude now-and-then a probable Consequence from appearing, provided it seems connected with some preceding manifest Truth; for such must be allow'd, as proper Foundations for a more exact Inquiry in a Matter I am very far from pretending to have exhausted. I must therefore observe, for my own Security against future Objections, that tho' I add no new decisive Experiment to my present List, or throw any more Light upon the Subject than what I have already amass'd, I may possibly, before my Essay appears, whether by the Advice of Friends, or otherwise, conceive more mature Thoughts, reject some of the present, and adopt others in their Place. As this will be done, without affecting in any degree the main System, which I imagine turns upon unquestionable Truths, it is a Liberty I am persuaded that equitable and learned *Society* will indulge me in, if no other Consideration prevails, than the great Obscurity that hangs over a Subject so extensive and so intricate as this is; in which I am already engag'd much farther than I at first foresaw, and indeed too far to recede without saying something.

§ 2. I shall take as little Notice as may be, in this short Summary, of the almost inevitable Mistakes others may have made in this Matter before me, and the too hasty Consequences they have drawn from Appearances that naturally surprise by their Novelty. Such Surprise is but too apt to captivate Persons even of the most serene Thoughts, much more the young and unexperienced; such as Mr. *Hartsoeker* was, when he first discover'd the spermatic Animals.

§ 3. Mr. *Lewenhoeck* indeed, so near his Cotemporary in this Discovery as to claim a Priority, was much more advanced in Age and Experience; yet if he should also appear to have been mistaken, we are not to be surprised at it; for his repeated Observations upon the Sperm of such a Variety of Animals, even as low as Insects, seem to intitle him to draw Consequences as extensive for a general System of Generation, as his Experiments had been. In effect, what two more powerful Arguments could a Philosopher with the Knowledge of no other Fact, than that of their Existence, have, than the Universality of Animalcules in this Fluid, and their seeming Confinement to this animal Secretion?

§ 4. The Method of Reasoning by Analogy is but too apt to lead us into Mistakes, and therefore we ought to be very diffident of Consequences deduced this Way. Every new Appearance that has no known Cause, immediately fixes, and; but toooften at last puts the Thoughts of the Observer upon the Rack. When the Mind arrives at this Intensity of Action, how natural is it to free ourselves from a painful Uncertainty at any rate, and that with as little Expence of Reflection as may be? The most obvious and easy Method is to class, if it admits it, and to reduce it to some other known *Phænomena*; possibly we are yet no nearer the physical Cause, because that of both is unknown. We have still, however, the Satisfaction to have diminished the Surprize it gives, by taking from its Singularity, and rest in some measure contented with this little Deceit.

§ 5. I call it a Deceit, if we acquiesce in it, till such time as a Number of Circumstances shall concur to

place it above the State of an Hypothesis, and shew us we have been right in our Inferences. Mere Analogy, founded only upon one or two Facts, and extended by Conjecture, however plausible, can but at most furnish Motives for a reasonable Doubt, and a more mature Enquiry. For tho', as a modern Author observes very well, Nature seems every-where to hold with itself, and go off by an almost imperceptible Gradation; yet, in our present Ignorance of the entire Chain of Beings, we are so liable to mistake two distant Species for the next immediate ones to each other, that the Analogy is thereby nearly extinguished, and its Traces almost effac'd.

§ 6. That this has been too much the Case in all the modern Systems of Generation, will appear I believe plain in the Course of this Memoir to every unbiass'd Naturalist. Animalcules were found universally in all animal Seed, almost at all times, and seemingly in this animal Secretion alone; they were therefore previously thought essential to Generation; or they should have added, a necessary Consequence of Properties in the Seed, which Properties were essential to Generation. But this Inference, however natural, was intirely overlook'd by them in their Reasoning; and Analogy induc'd them to stop at the first, without ever examining the second, tho' equally consequent. The Opinion of pre-existent Germs had prevail'd, under the Notion of Female Eggs, ere this Discovery was made; and thus one Mistake had been grafted upon another. When the spermaric Animals appeared, it was not difficult to transfer these imaginary Germs from the one to the other; and at most Philosophers were

only

only divided by it; tho' as both Opinions were equally plausible, the latter generally prevail'd by its Novelty. The vast and unbounded Prospect it open'd to the Imagination, in a View of such a prodigious Series from the first Parent to the last, of original Lineaments, struck the Mind with an agreeable Surprize. The Folly of equivocal Generation, particularly as it had been stated by the Antients, the false Grounds they had proceeded upon to establish it, various Experiments that seem'd to prove every Animal, every Plant, descended from Individuals of the same Species; but, above all, the Facility of classing these spermatic Animals, the reducing them by Analogy to Seed and Eggs, and the known Transition of most Insects from one State to another, seem'd all sufficient to remove the Veil Nature had drawn, and furnish a Clue of a competent Length to conduct us into its most hidden Recesses.

§ 7. Thus this new System of Generation soon became a favourite Opinion of the last Age, as it is indeed still of this for the most part; and many ingenious Methods were imagined of answering the Difficulties from Observation that seem'd to oppose it. The more antient Hypothesis of female Eggs was at last blended with it, and both were work'd up into one System: Their real Existence was determined, with their Form, Colour, Size, Situation, and the Mechanism of their Conveyance to the Womb; and imaginary Valves were appointed in each Egg admitting one, exclusive of every other spermatic Animal. Happy the first of these minute Beings that could take Possession of this little Cell, and shut the Door against contending Millions! Hitherto

therto every Step seemed easy and natural, if not too closely examined; the Inquisitive were conducted as high as their Curiosity could promise; and we might have expected, that Philosophers should have stopp'd here; but there is no End of reasoning by Analogy.

§ 8. No Body of Men so strictly deserves the Name of a Republic as that of the Learned does: Every one is passionately fond of adding to the common Stock, and claims nothing in Return, but the Name and Merit of having enrich'd it; yet this Passion is often so violent, that base Metals are mistaken for Gold, and Pebbles for Diamonds. It is not therefore Matter of much Surprize, if some have carried the imaginary Scene yet farther; and, still proceeding by Analogy, have supposed that the reticular Expansion, observed in the Womb of Does some Days after Copulation, by *Harvey*, and since him, in other impregnated Females, was nothing more than the investing Web, spun by the spermatie Animal before it enter'd the chrysalidal State, and preparatory to its Transition from one Form to another. Certainly these Authors never consider'd the immense Disproportion, between the great Expansion of this Web and the inconceivable Minuteness of the Animalcule; otherwise it had appeared as rational to suppose, that an *Alpine* Mountain could have been rear'd in a few Days by a single Emmet successively piling one Grain of Sand upon another. Nothing now seem'd wanting to complete this System, and place it above all Exception, but ocular Demonstration, if it might possibly be obtained, that the original Embryo was really contained

tained in each of these Animalcules: By Dissection, the young Butterfly had been observed in the Caterpillar three or four Days before it became a Chrysalid; Mr. *Lewenhoeck* had succeeded in some other very nice Operations upon extremely minute Subjects, nor did he despair of his Success in this; yet his repeated Attempts, it seems, all proved fruitless. But what the most exquisite Art had deny'd to *Lewenhoeck*, Chance, if we believe him, presented to another Naturalist, a little Man started from under the Integuments he was said to wear in his vermicular State; and the Observer very humourously gave us a Figure of this diminutive Entity perfect in every Member. These extraordinary Sallies, however, we must not place to the Account of the Learned, either of this or the last Age; they were generally exploded, and they indeed continue so; yet altho' they were peculiar only to the most lively; extravagant as they may appear to be, they were Consequences of the System; and thus was this Method of Reasoning by Analogy fairly pursued, as far as Imagination could carry it.

§ 9. *Cudworth, Grew, Le Clerc*, and some other Gentlemen of Judgment, had reflected too deeply upon Nature to give way to any Hypothesis, how plausible soever, that took in less than the whole Scene it exhibits to every attentive Observer. Yet they seem to have advanced much too far towards the other Extreme; and their System of plastic Natures, tho' in its Detail attended with many Proofs of extensive Thought, and profound Reflection, in a general View derogates as much from the Omnipotence

Omnipotence of an All-wise Creator; and is not perhaps less extraordinary, than that Opinion which attributed the Regularity and Motion of the Planets to the Ministry of Angels. In this Light, I presume, it has been looked upon by others, as well as by myself; and it is upon this account that I imagine it has had so few Followers; I shall therefore take no further notice of it here, than to observe, that, inasmuch as it admits a productive Force in Nature, and Operations that go much deeper than a mere Developement of Parts, it has certainly more of Truth in it, than the Opinion of pre-existent Germs: as I flatter myself, will appear evident in the Course of this Memoir, by Arguments drawn not from Observations only, that are obvious to every Naturalist, but particular Experiments made upon animal and vegetable Substances, during the whole Summer of this present Year.

§ 10. To enter therefore more particularly into my Subject, where to place the pre-existent animal Embryo, for instance, whether in the Animalcule or Egg, was ever the Question, and still remains unanswer'd. A Division of vital, essential, and original *Stamina* or Lineaments was impossible, yet innumerable Instances in Monsters, Mules, and many natural Subjects, concur to prove, that the young *Fœtus* partakes of the Nature, Qualities, Constitution, Form, and Features of both the Parents; even as far as their Defects and Diseases, which are but too often hereditary. How can it then be agreeable to Reason? Or to what Purpose should we call in to our Aid unalterable original *Stamina*? Can the visible Species of any Production be determin'd

min'd by them, if every sensible Quality may be influenc'd indiscriminately by either Parent? And if they cannot be alter'd, nor the visible Species be determin'd exactly by them, in what does their Essence consist, or how can they be applied to that very Use we seem to think them designed for? If they are placed in the Animalcule, or in the Egg, how are they transmitted? And if in the Animalcule, why is the Process attended with so vast an Expence, so great a Waste of Millions of Entities, each containing within itself a Series of the most perfect and most wonderful Productions in Nature, when one only of these Millions of Millions is alone to take Place? How are these Animals generated? if in the common Way, not only the Process will be boundless, and these in their Seed have others, and so on in an immense Series; but they can not then be unalterable, because they are supposed capable of being generated. Further, if they float in the Air, or lie hidden in Food, as some have thought, how is it that the *Stamina* of one Species do not sometimes insinuate themselves into a strange Parent, with all the Inconveniences and Absurdities of equivocal Generation? Or if they are said to be excluded by proper Strainers adapted for that very Purpose in distant Species; at least they cannot be so in those Kinds that are near a-kin: For if the spermatic Animal, which is naturally productive of a Horse in its own proper *Matrix*, is yet so fitted to the Eggs of the As, that it can possess a Cellule there exclusive of every other, which argues an exact Coaptitude, certainly the same Animalcules, if contained either in Food, Air, or Water, common to both Horse and As, might pass the

Strainers indiscriminately of either ; and thus might we have Mules common from each respective Male, without a promiscuous Congress of these two Species.

§ II. In another View, if we consider the extreme Tenuity, I may say the mere Nothingness of one of these *Stamina*, in its first Origin, at the Distance of many Ages ; comparatively to any one Part, the smallest muscular Fibre, for instance, of an adult Animal it is now said to constitute : how can we understand, that so minute a Filament could be developed, or in any Sense serve as a *Substratum* to a Cylinder so solid, so massive, so comparatively immense ? Could a Mountain be look'd upon as a Superstructure upon a Grain of Sand ? Or the terraqueous Globe derive its present Dimensions from the Dilatation of an Atom ? What is not the prodigious Force of this muscular Fibre in its present State, if compared with what it had in its Origin ? and, consequently, what must have been the Increase of real extraneous Matter, either by Apposition, or Incorporation ; which is now as much a Part of this Fibre as the original *Stamen* ? And if thus much can mechanically be assimilated, why not the whole of it formed by mechanical Causes ? Or why must so insignificant a Part of it be said to be concreated with the Universe ? But to strike at once with what, in my Opinion, may be look'd upon as a demonstrative Argument against the System of original *Stamina* ? The Difficulty still increases immensely, if we look into the Vegetation of Plants, and the wonderful Re-production of the Parts of Polypes, Starfish, Lobsters Claws, &c. The
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original *Stamina*, how minute soever, questionlets are diffused through the whole Production; since in this System all animal or vegetable Growth is made by Developement only: But if diffus'd, then some or all maybe by successive Bisection lost; and if lost, how can they be reproduc'd? Or if reproduc'd, why ever said to be original, and concreated with the Universe?

§ 12. These are but a few of those many Difficulties that might be enumerated; which yet are of such a Nature, that it is evident to every unbiass'd Observer, they cannot be even seemingly evaded, but by multiplying Suppositions on Suppositions; which at last render the Hypothesis so complex, as to retain no one Characteristic impress'd upon the ordinary Process and Operations of Nature. Is it not much more reasonable to say, that so many secretory Ducts, so many Strainers, so many preparatory Vessels in Animals, and such a curious Disposition in Plants for the Continuation of every Species, imply a Digestion, Secretion, and Preparation of Principles invariably, univocally productive of every Individual, when they fall into their respective *Matrices*, and find Aliment proper to assimilate? Are not these Principles contained in the Nourishment taken by the Parent Plant or Animal, the same that continually vegetate within it, and furnish it with Materials for its own Increase; continue to be distributed till it becomes adult, then plentifully exuberate, whilst it is, by new Preparations, fitted to propagate invariably in a proper *Matrix* its respective Kind? Else, why this Digestion? why this Secretion? why so many Strainers, Receivers, Ducts, and Valves? and why is some Food more productive of these Principles

ples than others? Or if they are pre-existent Germs that are secreted, are the pre-existent Germs of every Species contained in every Bird, Beast, Fish, or Plant, that supplies another with nutritive Juice, and becomes its Food? What a strange Confusion? How unlike that beautiful Simplicity, which Nature exhibits in all its Productions? Germs shut up within Germs, and Nature swarming with supernumerary Entities, all which we readily conceive might have been struck out at once, when the Universe was created; yet pretend not to be able to understand how they may be continually formed in Times successive, and as Occasions may require.

§ 13. This should seem as unnatural, and as unphilosophical, as it is disagreeable to Observation: For if every mix'd Body is made up by the Combination of certain Principles, I think we cannot question; but that God may have established Forces in Nature, subsisting Forces, by which such Principles may, in certain Circumstances, be invariably united, without any Danger of deviating, so as to render Generation equivocal; and if every Production in Question is a mix'd Body, as it certainly is, we know at the same time, that, how various soever they are, a small Number of Principles differently combined will yield an inconceivable Variety, sufficient to produce them all. Thus may we reduce Nature to what it is really ever found to be, simple in the Beginning of its Course, but magnificent beyond Expression when distributed: And this, I believe, will readily be allowed to be its true Process in Generation, if, besides taking in all the ordinary *Phænomena*, which no Hypothesis could yet explain,
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this Process is found consonant to many particular Experiments, some of which seem to me to render the System incontestable.

§ 14. Modern Naturalists have unanimously agreed to lay down, for a certain Truth, that every Plant proceeds from its specific Seed, every Animal from an Egg, or something analogous, pre-existent in a Parent of the same kind. If it is ever of Use to separate disagreeing Ideas, and previously to explain equivocal Words, it is particularly requisite in this Case to determine what we mean by Seeds and Eggs. Seeds and Eggs, in the common Acceptation of those Terms, are certain mix'd Bodies, of several Dimensions, that immediately furnish these Productions. In this Sense they are understood to contain not only the pre-existent Germ, but the *Nidus* also, if I may so term it, fitted for its Reception, and a due Supply of alimentary Principles to be assimilated in proper Circumstances. They are therefore thus far heterogeneous Bodies, that coalesce in a known Time; and their Principles are so far from being originally united at the Creation, that they sensibly come together from very distant Places in all hermaphrodite Plants, and from different Individuals in all those Species, where the Male and Female are distinct. Now I cannot persuade myself, but that either I have not understood what has been written on this Subject, or that Authors have not sufficiently reflected upon this, when they assert, that, because the *Plantula* is found in the Seed, an Oak, for instance, in an Acorn, that therefore this diminutive Tree bears likewise its Acorns, and thus on through a long Series.

Series. I shall not ask how this small Plant can have Seed ; in the common Acceptation of that Term, it is plain it cannot : and if it has not, where the pre-existent Germ is lodged ; how, from an Atom, at so immense a Remove, can it be increas'd to a sensible Mass, and be successively developed through so many Generations, till its Time of Appearance ? with many other Consequences that may be drawn from hence against the Reality of pre-existent Germs ; all which are too obvious to require a distinct Enumeration.

§ 15. It is in vain for us to pretend to lay down any one certain uniform Rule, and say to Nature, This is thy Scheme ; such are thy Statutes ; and from these thou shalt not deviate. If in many Productions she fixes it as an inviolable Law, that no Individual of that Species shall appear without a Co-operation of two Parents a Male and a Female, she has at the same time her Hermaphrodites both in Plants and Animals ; and if in these Hermaphrodites the two Sexes are yet so distinct, that she seems but to have a little diversified her Operations, without any sensible Deviation from her primitive Law, she will, in another Instance, that of the *Pucerons* observ'd by Mr. *Bonnet*, act either with or without the Co-operation of a Male. If again you say that a Female may be impregnated, so that the Impregnation shall diffuse itself, and penetrate as far as five or six Generations, she will point out to you in the Class of Polypes many Kinds, where Generation is carried on without either Male or Female, Egg or Seed ; tho', among these, there are some of the plumed Sort, where a whole Family, when by real Vegetation

getation branch'd out as far as Nature designs, jointly concurs to give one Egg, or something analogous to an Egg, as the Source of a future Progeny. And thus is this Class united to its next most immediate Superior. If you should still insist, that the vital essential *Stamina* of every Plant and Animal were really concreated with the Universe, and are now diffused in Water, Earth, or Air, from whence each will be united to its proper Subject in due time; or that the Experiments of *Niewentyt*, and other Naturalists, of the Stems and Roots of Beans, or other Seeds, altering their Directions several times when displac'd, to recover each its own, the Root downwards, and Stem upwards; that these I say evidently prove vital, essential, unchangeable *Stamina*; as they must be, if original, and concreated with the Universe: Instances might be brought from the Memoirs of the *Royal Academy of Sciences at Paris*, of Trees that have been so inverted, and induc'd to change their Direction, that the Branches have become Roots, and the Roots Branches; a *Phænomenon* totally inconsistent with vital, essential, and unalterable *Stamina*. In fine, if at last you resolve to stand by this one Resource, that at least every Individual proceeds from a Parent like itself; that the original Germs, tho' not wholly unchangeable, are yet sufficiently fix'd to determine every Species, and that they are either lodg'd in these Parents, or secreted from the Elements by Strainers through their Bodies: I believe I can turnish, from my last Summer's Observations, a Cloud of Instances, of a new Class of Beings, whose Origin has hitherto been unknown, wherein Animals grow upon, are produc'd by, and, in
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the strict Sense of the Word, brought forth from Plants; then by a strange Vicissitude again become Plants of another Kind, these again Animals of another, and thus on for a Series, further than the utmost Power of Glasses can carry the most inquisitive Observer.

§ 16. It has generally been thought by Naturalists, that microscopical Animalcules were generated from Eggs transported through the Air, or deposited by a Parent Fly, invisible to the naked Eye, or even that assisted with Microscopes. Yet is it strange that no Naturalist should yet have seen them, if they are really so numerous, when their supposed Progeny is so various, and themselves must be thought to be so frequently gliding over the Surface of all stagnant Waters. By what extraordinary Turn is it brought about, might a Naturalist observe, that such surprising Revolutions should happen in these little Oceans, as a total Disappearance of one Species followed by the almost immediate Succession of another; and that in a manner so sudden and unexpected, that I know not whither they are retired, or what new Forms they may have assumed. If they die, does a whole Race perish together, without any known Cause? Or if they have taken any new Form, how is it that I see none of them altering, just alter'd, or expanding their little Wings upon these Waters, wherein I lately saw so many Millions in an aquatic State? If it is possible for them to become flying Insects in a manner totally invisible, why do not these new Parents again deposit their Spawn in the same Waters, and give a Succession of the late Species, that has disappeared?

peared? The Element is not unfit for a new Progeny, since other Kinds succeed in it; nay I can transport from neighbouring Infusions some of the same specific Animalcules into these abandon'd Infusions, and they will live. Nor yet has the Generation of this Species any peculiar Season which confines it: A fresh Infusion of the same animal or vegetable Substance I apply'd before, will give me again in a little time the very Kind I am enquiring after, and that as often as I think proper to add new Matter. Thus might any Naturalist have reason'd, who had observed these Animalcules with some Attention; and been gradually conducted to doubt of their supposed Origin from flying Insects, or Eggs transported by the Winds.

§ 17. But there is yet a severer Difficulty, that springs from the Consideration of Paste-Eels: These Animals, Mr. *James Sherwood* and I, by performing a kind of cesarean Operation upon them, had the Pleasure to observe were viviparous; and the *Royal Society*, about the latter End of 1745, or Beginning of 1746, did us the Honour to give Attention to the Discovery, when Mr. *Sherwood's* Paper * was read, and the Experiments exhibited at one of its Meetings. I need not repeat what was at that time or has been since observed, where the Multiplication from one Eel once rose to 106. It is sufficient to observe, that these Animalcules musthence consequently be thought to have arrived at their ultimate State of Perfection; no longer liable to change, or to live in any other State; too weighty, even the least of them, to be buoy'd up by or transported

* See *Phil. Transf.* N^o. 478, p. 67.

transported through the Air, and too much of the aquatic kind to subsist out of Water, or to travel over dry Land, as I have often experienced, and any Gentleman may, by permitting the Water to evaporate. The Question therefore is, how, in a Mass from the clearest Spring-water, and the purest Wheat-Flour, heated as intensely as the Composition will admit, these Animalcules may be generated? It is not but that I think myself sufficiently enabled, by my Experiments and Observations, to answer all these Questions, and perhaps many more of greater Importance; but I have the strong Prejudice of near two learned Centuries, and the Opinions of Men of much more extensive Knowledge and Parts than myself, to stem and get over, before I can establish my own Sentiments upon this Subject; and therefore am willing to hope I shall not appear to have chosen a tedious and unnecessary Circuit, in tracing out the several Steps I have taken, to place my Conduct in a more rational Light. I must further observe, that I am obliged, previously to any of these Thoughts or Discoveries, to my Friend Mr. *Hill*, who translated and commented upon *Theophrastus* with so much Applause, for two Observations, made while I was at *London*, upon a Seed-Infusion he gave me, and the *Semen* of a Dog in his own House, which I, and some other Friends of the *Society*, saw; a Peculiarity singular enough was, that the Animalcules seem'd all hamper'd, and in some measure adhering by their suppoled Tails, struggling as it were with a kind of oscillatory Motion to disengage themselves, and not advancing at all progressively. The Consequence of this Observation, which sufficiently hinted that they were then enascent,

enafcent, and that their Tails were no Members given them by Nature to steer or swim wirhal, yet then escaped our Notice ; and was not plainly clear'd up, till other fimilar and more diftinct Observations upon this Clafs of Animalcules occurr'd fome confiderable Time after.

§ 18. It is now Time to obferve how much I am obliged to Mr. *de Buffon's* Penetration, who firft engaged me in this Enquiry, by his ingenious System, which he was pleas'd to read to me, and at the fame time expreffed his Defire I fhould purfue it, before I had myfelf any Thoughts of it, or any one Experiment had been try'd. He had been long difatisfy'd with the Opinion of pre-exiftent Germs in Nature ; and he and Mr. *Maupertuis*, Prefident of the *Academy of Sciences* at *Berlin*, had often difcourf'd together upon the Subject. We have feveral Hints of this Difatisfaction, in a little Book, published by Mr. *Maupertuis* himfelf upon this Queftion at *Paris*, before my Arrival there ; in fhort, it was, by general Reflections, and fome other conſequent Thoughts, that Mr. *de Buffon* was conducted to frame his System of organical Parts. Thefe he fuppoſed, by Coalition, to conſtitute the *prima Stamina* of all animal and vegetable Bodies, fimple, uniform, common to all, and conſequentially to be found in a certain Quantity in every Portion of Food, Aliment, or nutritive Juice ; and from thence to be digeſted, and when the Subject became adult, ſecreted, and ſtrain'd, for the Formation of the Seed of every Plant and Animal ; and in this Fluid or Subſtance to be conſequentially found in much Abundance. He further ſuppoſed theſe organical Parts to be moving when diſengaged, living in Appearance, and gifted with certain Organs, but

extremely simple in their Composition ; being perhaps little more than elastic Springs more or less compress'd, more or less diversify'd in the Direction of their Force. He thought the Calamary Machines I observed some time ago to be strong Proofs of his Opinion ; and the spermatic Animalcules to be Machines, or organical Parts like these.

§ 19. For my own part, I was then, as I had been before, so far of his Opinion, as to think there were compound Bodies in Nature, not rising above the Condition of Machines, which yet might seem to be alive, and spontaneous in their Motions ; such as the calamary Machines would certainly appear, if they were render'd so diminutive as to conceal their Mechanism, and such I then suspected the spermatic Animals to be : for Motion in general was but an equivocal Argument, and did not necessarily imply Life in the common Acceptation of that Term. When, for a further Proof, I instanced Mr. *Hill's* Seed-Infusion, wherein many Bodies were seen to move in a manner very different from Atoms in a fermenting Liquid, and yet not so seemingly spontaneous as microscopical Animalcules, he added, that in his System it must be so ; that these were detached organical Parts, and that the Seeds, and particularly the Germs of Seeds in Plants, must necessarily abound with them more than any other Substances. Thus did our Enquiry commence upon Seed-Infusions, from a Desire Mr. *de Buffon* had to find out the organical Parts, and I, if possible, to discover which among these moving Bodies were strictly to be look'd upon as Animals, and which to be accounted mere Machines. In the Course of this
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Paper I shall be as exact as possible, in philosophical Justice. Whatever Experiments or Discoveries are to be ascribed to Mr. *de Buffon*, were the Result of his Directions, or jointly made with him, I shall so specify, that they may appear distinguish'd from all those others I made at home. The four first Infusions, among them one of Almond-Germs carefully pick'd out from between the two Lobes and Kernel, I mixed up at my own Lodgings, and then clos'd them in Phials with Corks. The Observations that occur'd, were, first, a Separation or Digestion of the Parts of these Substances; and a continual flying off of the most volatile. These offuscated my Glasses at every Instant, and, according to the Mixtures, yielded a fetid or an agreeable Odour; particularly that of the Almond-Germs, one strongly spirituous. Eight Days after they had been infus'd, I began to perceive a languid Motion in some of the Seed-Particles, that before seem'd dead; such as gave me Encouragement to prosecute my Enquiry. It was visible, that the Motion, tho' it had then no one Characteristic of Spontaneity, yet sprung from an Effort of something teeming as it were within the Particle, and not from any Fermentation in the Liquid, or other extraneous Cause. A distinct Atom would often detach itself from others of the same or less Dimensions; and whilst these others remained absolutely unmov'd, advance progressively for the Space of eight or ten of its own Diameters, or move in a little Orbit, then fall off languid, rest between two others, and detach itself again and again, with a Continuation of the same Phenomena. The Consequences of these were obvious, the Motion was not spontaneous;
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for these Atoms avoided no Obstacle, nor had any other Characteristic of Spontaneity. It was not from any Commotion in the Fluid, Fermentation or the flying off of volatile Parts; because a large Atom would frequently move and detach itself from a much less absolutely quiescent: They did not seem to be enascent Embryo Animals, from a Deposition of any extraneous Spawn; for the Phials had been closed with Corks; nay they were the very Seed, or the Almond-Germ Particles themselves.

§ 20. These same Observations Mr. *de Buffon* made himself; for we examin'd these Infusions together a second time at his own House; and then it was that he order'd fifteen Seed-Infusions to be made up, which we continued regularly to examine twice a Week, till I propos'd to him to take them home, and follow them more closely by a daily or hourly Inspection; if necessary. The Result of our first Observations was, that tho' the Phials had been close stopp'd, and all Communication with the exterior Air prevented, yet, in about fifteen Days Time, the Infusions swarm'd with Clouds of moving Atoms, so small, and so prodigiously active; that tho' we made use of a Magnifier of not much above half a Line focal Distance, yet I am persuaded nothing but their vast Multitude render'd them visible. It seem'd therefore as if the first seeming languid Particles we had observed, vast in their Dimensions, if compar'd with those we now saw, had broke and divided into this immense Multitude of microscopical active Atoms. Then it was that we began to lay down a Distinction between animated and mere organiz'd Bodies; which, tho' far from being
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at this time groundless, yet afterwards proved to be false. These, and the spermatic Animals, we supposed to be of the latter kind; and to be produc'd in their respective Fluids, by a Coalition of active Principles, much as I had seen the Calamary Machines form'd by Hundreds, tho' absolutely detach'd, and swimming at Liberty in the Milt of the Fish: whilst we thought on the contrary, that the ordinary microscopical Animalcules, with strong Characteristics of spontaneous Motion and Animation, were to be class'd among Animals, and imagin'd them to proceed from Parent Individuals of their own Species. It was not till some time after this, that, determin'd to convince myself and others, without any Possibility of Doubt, whether these moving Atoms were really produced from without, or from the very Substance infus'd: I discover'd all the common microscopical Animalcules, the spermatic ones not excepted, were to be rang'd in the same Class, and that their Generation was very different from that of all other animated Beings.

§ 21. For my Purpose therefore, I took a Quantity of Mutton-Gravy hot from the Fire, and shut it up in a Phial, clos'd up with a Cork so well masticated, that my Precautions amounted to as much as if I had sealed my Phial hermetically. I thus effectually excluded the exterior Air, that it might not be said my moving Bodies drew their Origin from Insects, or Eggs floating in the Atmosphere. I would not instil any Water, lest, without giving it as intense a Degree of Heat, it might be thought these Productions were convey'd through that Element. Seeds or Plants were for this Reason

son improper, because they might have been judg'd to have been previously adhering to these Plants or Seeds: I neglected no Precaution, even as far as to heat violently in hot Ashes the Body of the Phial; that if any thing existed, even in that little Portion of Air which filled up the Neck, it might be destroy'd, and lose its productive Faculty. Nothing therefore could answer my Purpose of excluding every Objection, better than hot roast-Meat Gravy secur'd in this manner, and expos'd for some Days to the Summer-Heat: and as I was determin'd not to open it, till I might reasonably conclude, whether, by its own Principles, it was productive of any thing, I allow'd sufficient Time for that Purpose to this pure unmix'd Quintessence, if I may so call it, of an animal Body. From this time I take Corruption intirely in a philosophical Sense, for the rising of a dead Substance, by a new kind of Vegetation, into Life: and no Axiom, how much soever it may have been explod'd, is more true than that of the Antients, *Corruptio unius est Generatio alterius*; though they drew it from false Principles, and so established it as to render Generation equivocal, and never penetrated sufficiently into Nature by Microscopes, to discover this Class of Beings, that are neither generated nor generate in the common Way, yet furnish a Key to lead to the Generation of all others. My Phial swarm'd with Life, and microscopical Animals of most Dimensions, from some of the largest I had ever seen, to some of the least. The very first Drop I us'd, upon opening it, yielded me Multitudes perfectly form'd, animated, and spontaneous in all their Motions; And thus was I oblig'd to abandon not only the

Notion preconceiv'd of a Distinction to be made in this Class of Animals, between those that appeared under a sensible Angle in the Microscope, and the atomical ones; but even that Hypothesis also which I had advanc'd as probable, in the little Essay I published in 1745, that spermatic Animals were no more than Multitudes of such Machines as those of the Calamary; for now it was plain of what kind they were, and whence they deriv'd their Origin.

§ 22. I shall not at this present time trouble you with a Detail of Observations upon three or four Scores of different Infusions of animal and vegetable Substances, posterior to these upon Mutton-Gravy; all which constantly gave me the same Phenomena with little Variation, and were uniform in their general Result: These may better appear at Length upon some other Occasion; let it suffice for the present to take notice, that the Phials, clos'd or not clos'd, the Water previously boil'd or not boil'd, the Infusions permitted to teem, and then plac'd upon hot Ashes to destroy their Productions, or proceeding in their Vegetation without Intermission, appear'd to be so nearly the same, that, after a little time, I neglected every Precaution of this kind, as plainly unnecessary. I take no notice yet of their Manner of being generated and generating; in relating these Discoveries, as I believe I shall be more intelligible, if I follow the Order of Time: It is a Justice moreover I owe both to Mr. *de Buffon* and myself; for some were made by him alone, some by me, and some of them in Concert together: His System, the Detail of his System, his Experiments, my own Discoveries, my Thoughts

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in consequence of these Discoveries; all these were reciprocally communicated; we made a Secret of nothing to each other. Thus where one Truth seems to lead to; or is the natural Consequence of another, it will be easy, from the Order I have observ'd, to see how much I have been obliged to his Penetration and Foresight. But this will yet appear more distinctly, when our several Essays upon this Subject shall appear; and in the second Volume of his Natural History, which will very soon be published, I must declare for a Fact, that all which precedes his Accounts of the Experiments, begun *March* 16. *N. S.* of this present Year 1748, was previous either to his own Experiments or mine, and was read to me by himself.

§ 23. In this Order of Time therefore Mr. *de Buffon* not only repeated the Experiment I have taken notice of, and added particular Observations of his own; but made some intirely new in every respect, peculiar to himself. Among these, that never to be forgotten by Naturalists, which at once destroys the Opinion of Eggs in viviparous Animals, and shews the real Use of those reddish glandulous Bodies observed by *Vallisnieri* upon the Testicles or Ovaries, as hitherto call'd, of Cows. Every Anatomist knows, that the whitish Specks, near each of which a Hydatide is plac'd upon all Female Ovaries, were hitherto either look'd upon to contain the real Female Eggs, or to be the remaining Scars of Eggs fecundated and dislodg'd. *Vallisnieri*, nearer the Truth, thought the large reddish glandulous Bodies, which he calls Cherries, and found upon the Ovaries of Cows, and other Females, in the Time of their Heat, if the Animal is confined

to any particular Season, or at any Time, in those Females which are unconfined in this particular, were the real productive Organs contributory alone to Generation; yet still with a View to the antient Opinion of Eggs, for he supposed these glandulous Excrescences to be real oviparous Productions. Mr. *de Buffon*, on the contrary, long before Observation had realiz'd his Conjectures, rightly thought these to no more than temporary Blossoms, if I may so term them, not containing in their Cavity, which they have distinct when they are ripe, an Egg, but the real Female Seed; that the whitish Specks, scatter'd upon the Surface of Female Ovaries, were partly the remaining Scars of some of these temporary Blossoms now faded, as having perform'd their destin'd Office, or Embryo-Blossoms not yet expanded; that the Hydatid annexed to each of these contained a Quantity of imperfect indigested Seed; and that, if we took the Blossom in time, when it should be intirely ripe for Action, as when a Female is in Heat, or not barren, these red glandulous Excrescences would furnish a Fluid as really productive of true spermatic Animals, or organical Parts, as he calls them, as that of any Male observ'd by *Hartsoeker*, *Lewenhoeck*, or any other. The Result of these Conjectures was, that, ordering a Bitch in Heat to be strangled, and dissected immediately, we found two of these red Excrescences, florid and ripe, one upon each Ovary, these, from their respective Cavities that ran obliquely under these Productions for near an Inch in Length, furnish'd a Tea-spoonful of a thick turbid Fluid; and this Fluid, observ'd in the Microscope with the most powerful Magnifier,

after some little time exhibited Numbers of spermat^{ic} Animals, in every respect like to those hitherto observ'd by other Naturalists, animated, and moving spontaneously. Thus was Mr. *de Buffon's* Conjecture verify'd in every Particular.

§ 24. About this Time, I think some few Days after, Mr. *de Buffon* in my Presence examin'd several Sorts of male *Semen*; and then it was that, for the first time, we fairly saw the spermat^{ic} Animals enascent. Those Kinds which satisfy'd us in this particular were extremely viscid, and contain'd in a certain Quantity in the Chrystal of a Watch. These Precautions are not unnecessary; for if a viscid Kind be not chosen, and that in a good Quantity together, such as that of Stags, &c. or any Seed of the least exalted Sort, if I may so term it, as we found some to be more so than others; it will alter in the Atmosphere by an Evaporation of its volatile Parts, which serve to hold it though but gently together, after which it will liquefy, vegetate, ramify into Filaments, and these Filaments again break into moving Globules, especially if the Weather be hot, before a small Portion can be adjusted to the Microscope: whereby an Observer may easily be imposed upon, and think the spermat^{ic} Animals original and pre-existent, because he could not discern that Action which produc'd them. This Deception takes Place in all *Semen* of the more exalted Kinds, such as particularly the Milt of Fish, when it is in a State of immediate Impregnation, and many others: For it is to be observed, that the *Semen* of Animals is not at all times in an equal State of Exaltation; and consequently that some Sorts,
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or even the same at different times, will at some give the spermatie Animals immediately, but at others not so soon, and perhaps not under some Hours: which is the Reason why they have often been said by Naturalists, and even by *Lewenhoeck* himself, not to have been found upon Inspection. By this it will appear, that we had tried many Sorts, before we had the good Fortune to meet with one, in that exact Degree of Exaltation necessary to exhibit the whole Process of this Vegetation; and so may others who shall be desirous of trying these Experiments after us: Yet, when they shall at last have obtain'd a proper Subject, one accurate View will be sufficient, and found to give the Key to the whole Secret.

§ 25. When we had seized this favourable Opportunity, we saw a small Portion of male *Semen* plac'd on the Microscope, first, as it were to develope and liquefy, then shoot out into long Filaments, ramify on every Side, these open and divide into moving Globules, and trailing after them something like long Tails; these Tails were so far from being Members given them to swim and steer by, that they evidently caus'd in them an instable oscillatory Motion; and were in Effect nothing more than long Filaments of the viscid seminal Substance which they necessarily trail'd after them; they were of various Lengths in various Animals, and they insensibly, by the continual progressive Motion of those Animals, grew shorter and shorter, till some of them appear'd without any at all, swimming equably in the Fluid. It was then plain how these Animals were to be class'd; their Origin was clearly to be deriv'd from Principles contain'd in
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this Matter, either by an Evolution of organical Parts, as Mr. *de Buffon* supposed, or by a real Vegetation, as I thought, of the same kind with those I had before observ'd in my Infusions; tho' more prompt, because the Matter was more exalted: consequently the spermatic Animals were of the same kind as all other microscopical Animals, their Origin the same, their Influence nothing more in Generation, nor any otherwise conducting to its Cause, than as Effects of those Principles in the *Semen*, which alone are the true and adequate Cause of it. See *Fig. 1.*

These vegetative Powers, which, from the very Beginning of my Observations, I had found to reside in all Substances animal or vegetable, and in every Part of those Substances, as far as the smallest microscopical Point, I had at this time certain Proofs of; tho' not so plain and incontestable as those I procur'd a few Days before Mr. *de Buffon* left *Paris* for the Country, and which I prosecuted after his Departure. These I communicated to him in few Words the Night before he began his Journey, yet he was not at that time acquainted with any special Detail of the many Singularities that attend these latter Vegetations, for I had but just then made and enter'd upon the Discovery of them myself. I am obliged the more particularly to observe this, because the many Consequences he has since drawn, as well as myself, and which, without any mutual Communication, happen'd to tally with and seemingly to flow from the Discoveries, were not in Fact deduced from a circumstantiated Knowledge of these new Phenomena, which he had not, but from this one Principle, *that there*
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is a real productive Force in Nature; in which we had both long since agreed, however we may have differed in explaining that Action: For whether it be by an Evolution and Combination of organical Parts, as *Mr. de Buffon* supposes, or by a real vegetating Force residing in every microscopical Point, may be probably far beyond the Power of Microscopes to determine. But as the Principle from which we depart is intirely the same, it must necessarily lead to similar Thoughts, and similar Consequences.

§ 26. My first Proofs therefore were drawn from a close Attendance to all the common Infusions, particularly that of Wheat pounded in a marble Mortar. It was plain from them all, that after some time allow'd to the Water to call off the Salts and volatile Parts, which evaporated copiously, the Substance became softer, more divided, and more attenuated: To the naked Eye, or to the Touch, it appear'd a gelatinous Matter, but in the Microscope was seen to consist of innumerable Filaments; and then it was that the Substance was in its highest Point of Exaltation, just breaking, as I may say, into Life. These Filaments would swell from an interior Force so active, and so productive, that even before they resolved into, or shed any moving Globules, they were perfect Zoophytes teeming with Life, and Self-moving.

If any Particle was originally very small and spherical, as many among those of the pounded Seeds were, it was highly agreeable to observe its little Star-like Form with Rays diverging on all Sides, and every Ray moving with extreme Vivacity. The
Extremities

Extremities likewise of this gelatinous Substance exhibited the same Appearances, active beyond Expression, bringing forth, and parting continually with, moving progressive Particles of various Forms, spherical, oval, oblong, and cylindrical, which advanced in all Directions spontaneously, and were the true microscopical Animals so often observed by Naturalists. This brings to my Mind a Phenomenon often taken notice of, and seen with Surprise, Particles detach'd by the Reaction of the Water from the Extremities of the Fins of Mussels, which yet continue to move progressively. I think it sufficiently explain'd by these Observations; and that it is more than probable, that Mussels, Polypes, and other Kinds of this Nature, vegetate in a Manner analogous to this gelatinous Matter. See *Fig. 2.*

§ 27. In the Infusion of pounded Wheat, the first Appearances, after an Exhalation of volatile Parts, as in every other Infusion, were the second or third Day Clouds of moving Atoms, which I suppose to have been produced by a prompt Vegetation of the smallest and almost insensible Parts, and which requir'd not so long a Time to digest as the more gross. These in a Day or two more intirely disappeared; all was then quiet, and nothing to be seen, but dead irregularly formed Particles, absolutely unactive till about fourteen or fifteen Days after. From these uniting into one Mass sprung Filaments, Zoophytes all, and swelling from a Force lodged within each Fibre. These were in various States, just as this Force had happen'd to diversify them; some resembled Pearl-Necklaces, and were a kind of microscopical Coralloids; others were uniform throughout

throughout their whole Length, except just the very Extremity, which swell'd into a Head like a Reed, if the Force had acted equally on all Sides, or like the Head of a Bone at its Joint, if the Matter in its Expansion had bore to either Side. These Filaments were all Zoophytes, so teeming with Life, that whenever, upon taking a Drop from the Surface of this Infusion, I had separated the Extremity of a Filament so short as not to consist of above four or five Globules Chaplet-wise; they would advance progressively and in Concert, with a sort of vermicular Motion, for a little Way, then fall off irregularly to one Side, as if not yet fitted for progressive Motion, languidly turn their Extremities, and then again lie quiet for some little time. It was my Fortune however, not in this Infusion only, but in many others, to find some of these Chaplet-like Animals much smaller indeed than those of the Wheat-Infusion; but intirely regular, constant in their vermicular Motion, and which were consequently arrived to a higher Degree of Maturity and Perfection. I own I cannot but wonder to this Day at what I saw; and tho' I have now seen them so often, I still look upon them with new Surprise. Yet have these Phænomena serv'd me to very good purpose, and clear'd up many Difficulties in my former Observations.

The Origin of Blight in Wheat, Rye, and other Vegetables, was no longer mysterious: An Atmosphere charg'd to an extraordinary Degree with Humidity, now plainly appear'd sufficient, particularly while the Grains were tender and replete with a milky
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Juice

Juice in a certain Degree of Exaltation, to produce in them this new kind of Vegetation, and to form their interior Substance into Filaments, which are indeed those very Eels I observ'd some Years ago in blighted Wheat.

This agrees perfectly with another Observation made by the Gentleman who translated my little Essay into *French*: Some of this blighted Wheat, two Years after I had gather'd it, I had given to Mr. *Trembley*, and he to this Gentleman. In a Note he has added, he observes, that these Filaments not only recover'd Life and Motion, after they had been so long dry, by macerating them in Water; but many broke, and discharg'd from within them Globules, which mov'd with extreme Vivacity. The Application of the foregoing Observations to this Case is easy and natural; nor is it now any Wonder, that these Filaments, the vegetative Force still residing within them, should move and resolve into Globules, or that they should have subsisted so long, full of that kind of Life they are actuated with, though dry and without Nourishment; for now they cease to be Eels, as I formerly thought them.

Blighted Rye, which is also so full of Filaments of this Nature, that the Grains are swell'd in their Diameters, and extended to an extraordinary Length by this new kind of Vegetation, exhibited nearly the same Phænomena when macerated, and is to be class'd accordingly. I am told by some of the Gentlemen of the *Royal Academy of Sciences* here, that in those Provinces of *France*, where this blighted Rye abounds, and is made up into Bread; it produces very strange Effects in the poor Country People who feed

feed upon it, many of which are here found in the Hospitals afflicted with a very singular kind of Mortification, which causes their Limbs to drop off.

There are two Sorts of Blight, in one of which the Grain crumbles into a black Powder; and the other is that which gives these moving Filaments or Eels. Mr. *Bernard de Jussieu* tells me, that one is from a Corruption of the Flour, and the other of the Grain.

It may not here be amiss to hazard a few Queries. Do not all Mortifications, and other Maladies in which there appears an extraordinary Exuberance of Matter in any one Part, proceed from a Weakness, a Want of Resistance, and from Principles of Union, which give to this vegetative Force, found to reside in every Point of animal or vegetable Substances, more Play in one Part than in another? For If the Resistance be not equal in all Parts, the exuberant Matter must break forth, and cause that Part to decompose; and if the Habit of Body be extremely lax, the Decomposition must continue; and that, in a certain extraordinary Degree, we shall call a Mortification. To rub a Wound, or any natural Sore, with Salt and Spirits, is found to be salutary, and preventive of Mortifications; and Salt I know, by Observation, will immediately put a Stop to these microscopical Vegetations, and cause the Animals to subside motionless to the Bottom: There fore it is probable, that Salts and Spirits are Principles of Union, and productive of a greater Resistance in the ductile Matter acted upon by this vegetative Force. High Living, rich Wines, &c. are Preservatives against many contagious epidemical Distempers; Do not therefore these Maladies arise from a laxer

Habit of Body, and a more than ordinary Action of this same vegetative Force? And may not these, and many other Phænomena of this kind, be reduc'd to the same Principles? But this I leave to the Consideration of Physicians, who are better Judges of the Extent of these Observations and Principles.

The Substance emitted from the Globules of the *Farina fœcundans* of all Flowers, by an Action I observed some Years ago, is also a Substance of this Nature, filamentous, and in a vegetating State: Nothing can resemble it more than the Fibres of most kinds of Mould; resolving all, as they do in Water, into others of a much finer Contexture, when the Vegetation, that had been before stopped by the nitrous Salts of the Atmosphere, begins by the Assistance of the Water to act again: And I know, by Observation, that all kind of Mould is formed by a Process of the same Nature as the Growth of these microscopical Plants; and to be class'd consequently with them, and reduc'd to the same Principles.

I cannot finish this Article without observing, that nothing can more perfectly than these wheaten Filaments, represent in Miniature Corals, Coralloids, and other Sea Plants, which have long been observ'd to be teeming also with Life, and have been suppos'd to be the Work of Animals, as it will appear to any one, that but inspects the Figure I have annex'd, and recollects my Description. Are not therefore all these in the same Class, and is not their Origin similar? See *Fig. 2.*

§ 28. But these Instances from common Infusions, of a vegetative Force residing in every microscopical Point of animal or vegetable Matter, how strong

strong soever and surprizing, were neither so wonderful or extraordinary as some others I observ'd after Mr. *de Buffon's* Departure. From the wheaten filamentous Zoophytes it was easy to infer, that they sprung from, and were Productions of, the Mass of Matter that had subsided to the Bottom of the Phial. Yet this I could not obtain a Sight of; nor was it possible in this Way to observe them without separating them from their Roots and from the Mass, out of which they arose. The Method the most natural therefore which occur'd to me for the viewing of these Zoophytes, without disturbing their Vegetation, and for observing their whole Process, from the Origin of the Plants to their last Degree of Maturity, was to take extreme thin Slices of Cork, and insert, through little Holes which I made, four or five in each Slice, Grains of Wheat or Barley, or any other farinaceous Seed, for these all nearly agree in the Phænomena they exhibit, with the Germ either turned upwards, or carefully pick'd out with the Point of a Penknife, to prevent their usual shooting.

These were permitted to swim upon the Surface of fresh Spring-water, in a Glass exposed to the Sun, that the whole vegetating Force might be determin'd downwards towards the inferior Moiety of each Grain, which alone could in these Circumstances imbibe and be saturated with Moisture. This answer'd my Purpose intirely; my Plants grew downwards into the Water like Corals, but appear'd not till several Days after the Grains had been thus expos'd; and were at last so large and strong, that I could see them with my naked Eye.

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When they became thus visible, I cut off with a small Pair of Scissars the vegetating Extremity, and plac'd it in a concave Object-Glass with Water. The Plants then took a new Direction, follow'd the Expanse of the Fluid, and continued to vegetate, while I supplied them with Water, which I did from time to time, covering them after Observation with another concave Object-Glass, to prevent the Fluid from evaporating too fast. Thus I had for the Subject of my Observations what I may call a microscopical Island, whose Plants and Animals soon become so familiar to me, that I knew every animal Species, and every individual Plant almost without any Danger of Mistake; an Exactness so necessary, that it would not otherwise have been possible to follow the Process of this Vegetation without Confusion. From this time I laid aside the Use of large Infusions, and provided a certain Number of Watch-Chrystals, or concave Object-Glasses, for every Portion of animal or vegetable Substance I was to macerate in Water. The Use of these is plain and easy; many fruitful little Islands of various Kinds with Labels and Dates affix'd to each may thus be obtain'd, by placing the vegetating Substances in these Glasses, and this is the Method I would recommend to all those who shall be desirous to repeat or pursue my Experiments.

I find my Subject grows upon my Hands, and I am unwilling to take up too much of your Attention: I shall therefore finish these Observations by annexing a Figure of my Wheat-Island and its Productions, all which will be sufficiently intelligible without any more Words; and I shall reserve a

Multitude

Multitude of other Observations I have by me in my Journals, upon Infusions and other vegetating Islands for the Essay, which I hope to publish in some Months, if these few Thoughts and Discoveries shall meet with Approbation. See *Fig. 3.*

§ 29. Yet must I trespass for a few Pages more; I cannot conclude this Letter without laying down some general Truths, and recalling these scatter'd Remarks to some certain Principles. A few Propositions of this kind, together with the probable Consequences, that seem naturally to flow from them, will not only make my System of Generation clear, but also take off many Objections, and render these very Observations better understood, when they are reduced under certain Heads.

It seems plain therefore, that there is a vegetative Force in every microscopical Point of Matter, and every visible Filament of which the whole animal or vegetable Texture consists: And probably this Force extends much farther; for not only in all my Observations, the whole Substance, after a certain Separation of Salts and volatile Parts, divided into Filaments, and vegetated into numberless Zoophytes, which yielded all the several Species of common microscopical Animals; but these very Animals also, after a certain time, subsided to the Bottom, became motionless, resolv'd again into a gelatinous filamentous Substance, and gave Zoophytes and Animals of a lesser Species.

This is not only true of all the common microscopical Animalcules, but of the spermatic also; which, after losing their Motion, and sinking to the Bottom, again resolv'd into Filaments, and again gave lesser Animals.

Animals. Thus the Process went on through all visible Degrees, till I could not any longer pursue them with my Glasses: And thus evidently the spermatick are to be class'd with the common microscopical Animals.

Hence it is probable, that every animal or vegetable Substance advances as fast as it can in its Resolution to return by a slow Descent to one common Principle, the Source of all, a kind of universal *Semen*; whence its Atoms may return again, and ascend to a new Life. This common Element therefore, tho' uniform in its Origin and homogeneous, branches out into innumerable Species more and more compounded, more and more heterogeneous, as they depart and are further from this Source of organiz'd Bodies; yet may a Particle often be arrested, or moulded into other Bodies; long before it attains, which some perhaps never do, to this ultimate Resolution. Nor is there any Danger upon these Suppositions of falling into equivocal Generation; because the specific *Semen* of one Animal can never be moulded into another, and Seeds may differ specifically from one another by many invisible Principles totally unknown to us, and unattainable by Experiments; for we are very certain that the Power of Glasses, or Force of any *Menstruum* we can employ, must still leave us at an immense Distance from the ultimate Resolution of Bodies, in which alone they agree, and are homogeneous.

I say therefore the specific Seed of one Animal can never give another of a different Species; for, to be this *specific Seed*, it must have gone through many Changes from its first Origin, and have many Singularities peculiar

peculiar to itself, and acquired, since it passed from the homogeneous Element, in which all Kinds coincide. The active vegetative Force that resides in it must be precise, its Quantity must be exactly proportion'd to the Nature, Solidity, Tenacity, Quantity, and Resistance of the ductile Matter it has to wade through, if I may so express myself; and these Combinations are very different in different Subjects. Thus much the many Strainers in every animal Body, necessary to extract this *Semen* from the Aliment we daily digest, and to prepare it, seem evidently to imply. Yet is not this sufficient, as it may appear to cause Varieties in the several Species of *Semen*, all that is to be consider'd: Times and Circumstances make Changes in it even during the Term of Gestation. What does not the *Fœtus* then undergo? and who can determine the Differences between *Matrix* and *Matrix*; between the Matter that is assimilated into a *Fœtus* in one Subject, and that in another; between the fixing Principles, the Quantity of Salts, Spirits, &c. in a Parent of one Species, and one of another; between the more copious or more limited Affluences of assimilating Matter; and between Times, where even single Minutes, Instants, &c. may be of the greatest Consequence? I see the Whole indeed, but confusedly; yet do I see the Source of a Variety; which, boundless as it were, if permitted to expatiate at full Liberty, is nevertheless invariably confin'd, by Him who made and rules the Universe, to a certain determinate Number of Species. Time, Action, Season, Quantity of Force, Quantity of Resistance, fixing Principles, Affluence of assimilated Matter, Direction,

and numberless other Variations, are all employ'd for His Purposes, and modell'd by that Almighty Power, which forms and directs the Whole.

Thus do these Principles, however capable of differing Combinations, yet admit only of a limited Variation, and never deviate further than is consistent with univocal Generation. Monsters, Mules, imperfect *Fœtus's*, and other Instances of this kind, are but rare; and as they can be ascrib'd to nothing so properly as to the Obstacles they meet with, or to some accidental insuperable Resistance in the Matter of which they are formed, they do at least serve to shew that there is in Nature a real productive Force given it at its Creation; and that animal or vegetable Productions are not the Consequences of pre-existent Germs, plastic Natures, or of the immediate Hand of God himself, any more than the most regular Operations of the Planetary World.

§ 30. But to proceed in my Consequences from these Observations, all Naturalists must acknowledge, that the more compounded the organiz'd Bodies are, the less Danger there is of equivocal Generation in the Production of them; for thus the immediate Principles from which they spring, and their Circumstances during the Time of Gestation, must be much more varied than the more simple Bodies are, and at the same time be further removed, from that universal Element into which they may all ultimately be resolved: And even in the lowest Class of microscopical Animals, I can truly say, that I never yet observed any others than Productions specifically determin'd; the same Substances giving the same Plants and Animals, and in the same uniform Order

der and Descent. Nevertheless, tho' thus specifically determin'd, no one, that observes their Origin with the same Care as I have done, will be inclined to ascribe it to pre-existent Germs; It is therefore probable, as I just now advanced, that when we arrive at the lowest we can discover in this Class, we are yet at an immense Remove from the universal Source; notwithstanding that some of them are small beyond Conception, and no less simple in their Motions; which argues their Organization as simple, and seems to imply that there are among them, or not at a very great Distance from them, such as are only mere Machines, without any true Spontaneity.

I have myself seen a vast Gradation, and such a one as I have yet but an imperfect Notion of; in a Course of continual Observations made upon Infusions and Macerations of all kinds, from the most compounded to the most simple; from Animals of the largest kind to moving Atoms of the least; from Motions as slow to the most powerful Magnifier, as the Motion of the Minute-Hand of a Watch to Eyes unarm'd; from free Progression in all Directions to merely oscillatory Balances; which all seem to come to at last in the Course of their Decomposition, when they are just upon the Point of disappearing.

§ 31. Thus these Animalcules, if they may be call'd indifferently by that Name, manifestly constitute a Class apart; and their greatest Characteristic is, that they neither are generated, subsist by Nutrient, as other Plants and Animals do, or generate in the ordinary Way. This is indeed true, if the whole Class is taken in one general View: Yet is the Head of it united to the Species of the next im-

mediate Superior. The Bell-Animal, of which I have had many from my infused Substances, and whose Growth I have pursued from its first Origin, is a Species of microscopical Polype, generating and feeding as other Polypes do, when once itself is generated; tho' its own original Generation is perhaps different from that of the others; for I could never obtain any of the larger Kinds this Way. I say this however with some Reserve; for I will not assert but that some decay'd Water-Plants decomposing in particular Circumstances, and their Substance exuberating, may perhaps, when urged by this vegetative Force, give Polypes of every Kind; nay I very much suspect, that several of the lowest Kinds of visible Animals may, In due Circumstances, which yet perhaps are rare, be recoverable this Way, when the whole Species has perished in particular Places by some uncommon Accident. This I the more readily believe, from the Reasonableness of some Allowances to be made in this respect; all which may be permitted; and must have been foreseen by the GREAT CREATOR, without any Danger of Confusion, or an unlimited Generation of new Species never before produced: He who made Nature, and sees through the whole Machine, well knew its utmost Force, and has consequently foreseen every Circumstance, and limited its Productions accordingly.

Nor indeed can there be a stronger Argument deriv'd from any System of Generation whatsoever, of an All-wise Being, All-powerful, and All-good, who gave to Nature its original Force, and now presides over it, than from the Consideration of an exuberating ductile Matter, actuated with a vegetative

tive Force, limited, tho' we know not its exact Bounds, in its specific Ascent or Descent, and expanding itself in Directions as certain and determinate, as the Motions of the Plants.

§ 32. These Thoughts will appear to be less hazarded, if due Attention is given to the Generation of the Paste-Eel. The *Royal Society* knows it to be viviparous; consequently perfect in this State, and such as may continue to generate in the common Way, as long as it has an Element and Matter proper for its Subistence; yet is its own original Generation, as far as I can learn by Observation, as that of all these microscopical Animalcules, from a ductile vegetating Matter, the Produce of Wheat-Flour and Water; tho' it undergoes more Changes than others, and lives in other Conditions; ascending for some time before it enters its chrysalidal or Egg like State, whence it comes forth a perfect Eel. I have added a Figure of a Group of these Eel-Chrysalids, but the Detail of their Metamorphosis I shall reserve for my little Essay, and not trouble you now with an Account too circumstantiated of every Observation I have made upon them: Besides that I am not yet thoroughly satisfied in the whole Manner and Process of their Generation. See TAB. V. *Fig. 4.*

§ 33. But now, to obviate every Objection that may remain against the Existence of this vegetative Force, which seems to be the Key to much Knowledge, and to remove many Errors; it may be proper to add, that, besides ocular Demonstration, which any Naturalist may have, besides the Precautions I took, that no supposed Germs might either be convey'd through the Air or the Water, or remain ad-
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hering to the Substances infus'd; I have often, for these Purposes, made use not only of hot Broth, immediately clos'd up in a Phial, but also of pure animal Substances, such as Urine, Blood, &c. with the same Success; and in these, I believe, no one will suppose that Germs, Eggs, or Spawn, are pre contain'd, if Care is taken to close the Phials immediately.

Nay I have done more; I have, by reasoning consequently to my Principles, been directed to the Choice of many Experiments, all which I constantly found to answer my Expectation: I have thought, for instance, that the more exalted an animal Substance was, by a certain Degree of Decomposition, the more apt would it be to vegetate in a proper Matrix, and form the Part of a larger Animal; or, if it extravasated, to vegetate into the lesser; consequently, that if I took the milky Juice of germinating Seeds, or that thick turbid Matter which forms the Wing of a Butterfly in its chrysalidal State, these Matters must be more exalted than any ordinary Substances, and therefore give me these microscopical Productions so much the sooner: And in fact, I never, in these Cases, fail'd of seeing them within the Space of a few Hours, while ordinary Infusions did not give them under several Days.

Here it will be proper to observe, that Naturalists have thought the Butterfly's Wing pre-existent in the Caterpillar, because they discover'd the first Rudiments of it three or four Days before it enter'd the chrysalidal State; but it is then precisely that the Caterpillar first leaves off eating, tho' before extremely voracious; and that probably upon account

count of the Revolution it finds in all its Parts, while its Forces are otherwise employ'd, and the Collection of vegetating ductile Matter it had acquir'd by plentiful Diet, now as plentifully exuberates to form the Parts of the Butterfly. These Truths I am the better acquainted with, because I have particularly examin'd all those Substances: You cannot tear off a Portion of the Butterfly's Wing, even while in the Chrysalid, but you will find it in an Embryo-State, and the Matter which extravasates upon your Object-Glass, if mixt with a little Water to preserve its Fluidity, will almost immediately vegetate into these microscopical Productions. This argues an extreme Activity in it; from Activity follows Action, and an Effect, which can be no other than the Formation of the Wing it was contain'd in.

§ 34.. Without instancing in many other Examples, where, by reasoning from these Principles, I was invariably conducted to certain Consequences, this last sufficiently leads to the Nature of animal or vegetable *Semen*. These latter are Substances of the same sort, but more exalted, and from thence adapted to a prompter Vegetation.. Of this kind also, but not so exalted, was the gelatinous Substance I obtain'd by common Infusions.

The Exaltation however of Matter does not stop here; the lower I pursued this new Class of Beings in its Descent, the less was this vegetating Force clogg'd with resisting Matter, the swifter was the Motion of the Bodies, and the higher the Degree of Exaltation that produc'd them. This inclines me to believe, that an animal Substance may be exalted
this

this way into a Poison, a Venom, or a contagious Vapour. Hence stagnating Waters are poisonous and detrimental; and hence perhaps the vipereal Venom, or any other, may derive its Force; for these undoubtedly are all animal Secretions. Hence perhaps also arise contagious epidemical Distempers, from a Leaven thrown into the Blood by Exhalations of this kind. I am the more persuaded of the Truth of this, from the Consideration of Dr. *Mead's* Observations upon the Venom of the Viper: And swift moving Bodies, which subside and shoot into Filaments, seem manifestly to imply all these Consequences. I had myself propos'd last Summer to try the Effects of some of my most exalted Infusions, by instilling them into the Veins of Animals; but as yet I have had no Opportunities for these Experiments.

I might add other plausible Conjectures, that seem to be the natural Consequences of these Discoveries, relating to the Origin of *Ascarides*, *Teniae*, *Agaricks*, &c. nay, perhaps I could maintain them with Arguments that would seem convincing to most Naturalists; I might even further suppose, with some Probability, that the muscular Force, which acts against the interstitial Air in my Friend Dr. *Parsons's* most ingenious System, in one Word, that all the mechanical Forces of the Body, and the Impressions which affect the Soul, may be derived from, and ascribed to, this vegetating active Force when confined: But I am tired with extending my Views so far, nor do I at present see an End of the Consequences; the Subject and Principles appear so boundless.

§ 35. I shall conclude therefore with summing up my System in a few Words: I suppose all *Semen* of any kind to be an exalted Portion of animal or vegetable Matter, secreted from the Aliment of every generating Subject, when it is adult, and no further Demand is made for its Increase and Growth; this I suppose to be endued with a proportionable vegetative Force; to be various in various Circumstances, and heterogeneous in different Subjects; but to be uniform in its Productions, when it falls into a proper *Matrix*, where it finds Matter to assimilate, of a Quality and in a Quantity sufficient to form that specific Being; whilst in other Circumstances, it will, if it extravasates, by the same vegetating Force, yield all the several *Phænomena* I have above taken notice of. And thus, if I am not mistaken, I have obtained what I first intended to make out, that the spermatic Animals are not the efficient Cause of Generation, but only a necessary Consequence of Principles in the *Semen*, which Principles are necessary to Generation.

Thus have I connected my System with our Countryman Dr. *Harvey's* Observation of that fine Tissue; or Web-like Expansion; observ'd in the *Uterus* of Does, in the Center of which the Embryo *Fætus*, invested with its *Amnion* and *Chorion*, was found to be lodg'd: For let the Vegetation begin from the *Semen*, and continue to assimilate the affluent Matter from the *Matrix* wherein it has taken Root, and the Fawn must come forth like any other specific Animal or Plant.

I shall only observe, that *Lewenhoeck* had discover'd this vegetating Power in the *Semen*, and had,

like Mr. *de Buffon* and me, seen the Filaments from whence the spermatic Animals spring; he even calls them Nerves and Arteries; and in one of his Letters to Mr. *Oldenburg* says, that he saw more in one Minute than the most accurate Anatomist could discover by Dissection in a Day: But when he afterwards chang'd this System, false as it was, of Nerves and Arteries for another, I believe, as false, that of pre-existing Germs in the spermatic Animals, he neglected to improve this Observation as he might have done; nay he afterwards took no farther Notice of it, but barely to say, that it was to be neglected. This Remark I had from Mr. *de Buffon*.

The Difference therefore betwixt Mr. *Lewenhoeck* and Dr. *Harvey* was, that the first had an Hypothesis to maintain, and the latter nothing in View but to follow Nature, without trusting too much to the first *Phænomena*, as I hope I shall appear to have done in this my Enquiry.

I had almost forget one Remark that coincides with my System; that although animal and vegetable Substances by a chymical Analysis appear to differ, they are nevertheless found by a natural Corruption to be reducible to the same Principles. This has been observed long ago by many Naturalists.

And now I think I have nothing more to add, only that I would be understood, when I speak of a productive Force in Nature, &c. to mean only a Force, which, tho' modell'd by the SUPREME CREATOR, goes no further than the mechanical and material Parts of a Man. I well know that we are composed of two very different Principles; and no one mere philosophical Truth whatsoever presents
itself

itself to me with more Evidence or Conviction than the Spirituality of our immortal Soul. All have ever allow'd Man in his Origin to be a kind of Plant or Vegetable before he is animated; and all rational Men have deriv'd his Animation immediately from the Fountain of Life, the true Source of all spiritual Substances. I think I have said no more; and thus only would be taken and explain'd.

The Principle of Life in other Animals I do not examine into, nor do I think it necessary. If they are truly spontaneous, as they seem to be, they have certainly some Principle distinct from Matter, which the GREAT CREATOR knows when and how to unite.

This Exposition, Sir, of my Sentiments, I thought might be necessary; not that I imagined that either you, or any of the Gentlemen of the learned *Society* in which you preside, would think my Principles any way tending to Materialism, from which no one can be more distant or averse than myself; for I well knew that I had nothing to apprehend from Persons of so much Judgment and Discernment, and who could not but clearly see, that there is really no Connection between those Principles, rightly explain'd, and the Doctrine of the Materialists: But I was willing to guard against the Misapprehension of others less acquainted with Matters of this sort, and into whose Hands this Paper might come, and have therefore taken these Precautions.

And now, Sir, I take this Occasion of returning my most humble Thanks both to yourself, and to the rest of the Gentlemen of the *Royal Society*, for the Honour I have received, in being elected one of its Members, and for which I have not been

able as yet to make my personal Acknowledgments I hope both you and they will accept these Thoughts favourably, which are humbly submitted to impartial Inquiry by the Author, who is, with the utmost Esteem and Respect,

S I R,

Your obliged humble Servant,

Turbervill Needham.

Explanation of the Figures in TAB. V.

Fig. 1. Represents the Origin of the spermatic Animals.

Fig. 2. The Wheat-Infusion.

Fig. 3. What I have called an Island in the Wheat-Infusion.

Fig. 4. A Groupe of the Chrysalids of the Paste-Eels.

Fig. 5. Is a Draught of one of the first microscopical Plants or Zoophytes which I discover'd; wherein *A* shews the Figure of the Plant throwing out its Animals, and *B* the same again after the Animals were discharged, again putting out a new Shoot from the Stem below, through the hollow transparent Head, to form a new Head, and produce another Generation.

VII. Obs.

VII. Observationes astronomicae *variae factae*
in Paraquaria, Regione Americae Australis,
ab anno 1706 ad annum 1730. quas cum
Regali Societati communicavit Jacobus de
Castro Sarmento M. D. Coll. Lond. Lic.
 & R. S. S.

Presented Jan. 28.
 1747-8.

ECLIPSES Solis et Lunæ observatæ
in Missionibus Paraquariæ, Soc. Jesu
a P. Bonaventura Suarez ejusdem Societatis Missio-
nario, adhibito Telescopio quinque Pedum, et oscil-
latorio minuta secunda exhibente; motu aequali, et
per altitudinem Fixarum ad Tempus verum recti-
ficato.

Eclipsis Solis, anno 1706, Nov. 5. in oppido *Sancti*
Ignatii ad Paraquariam, cujus altitudo poli austr.
 est 26°, 52', ejusque differentia meridiana ab observ.
 regio *Parisiensi* horar. 3. min. 57. sec. 50.

		Stylo civili
Initium eclipsos	8. 52	Manè ante merid.
Digiti obscurati 2	9 15	
	3½ 9 40	
	4 10 0	
	1 11 5	
Finis	11 15	

Maxima quantitas ad hor. 9. m. 50. dig. 4. 0'.

Eclipsis Solis ibidem observata anno 1709, *Martii*
 11, Stylo civili.

Initium infra horizontem : ortus solis ibi hor. 5.
 53'. Digitis

				Manè ante merid.
Digiti obcurati	9	20	6 15	
	6	30	6 50	
	6	0	6 54	
	5	0	7 3	
	3	30	7 13	
	3	0	7 17	
	3	30	7 21	
	1	30	7 28	
Finis eclipseos			7 37 15"	
Oculari lente tubo fumo infecta.				
Fuit ejus maxima obcuratio digit. 9 26.				

Eclipsis Lunæ ibidem observata anno 1707, *Aprilis*
16, post meridiem.

Initium	7 55
Totalis obcuratio	8 58
Initium emerfionis	10 45
Finis non est obfervatus ob nubes.	

Eclipsis Lunæ ibidem observata anno 1708, *April. 4,*
post meridiem.

<i>Immerfio Lunæ</i>				<i>Emerfio Lunæ.</i>			
In penumb. fenfib.	12	18	0	Aristarch.	14	13	15
In umbram	12	30	29	Plato	14	45	0
Aristarch. obfc.	12	37	11	Ex umbr.	15	3	0
Plato obfcur.	12	46	0	Ex pen.	15	12	0
Serenò cœlo.							

Eclipsis

Eclipsis Solis ibidem observata anno 1730, Jan. 18,
post meridiem.

	<i>h</i>	<i>m</i>	<i>s</i>	<i>Digiti obscurati.</i>	
Initium	2	52	30	0	0
	2	58	10	1	0
	3	5	0	2	0
	3	19	45	4	15
	3	29	20	5	45
	3	21	22	6	0
	3	39	17	7	0
	3	41	55	7	20
	3	45	40	7	40
Nubes.					
	4	7	33	8	0
	4	9	36	7	45
	4	11	34	7	30
Nubes.					
	4	31	0	4	0
	4	42	0	2	0
	4	50	0	0	30

Finis non est observatus ob nubes; videtur fuisse
hor. 4, 52', fere hora 4, 55'; discus solis integer
visus; nec luna apparebat in ejus limbo.

Maxima obscuratio videtur fuisse digit. 8 $\frac{1}{2}$.

Anno 1729, nubilo coelo *Augusti* 8, in eclipsi totali
Lunæ post mer. hæc tantum observavi, in oppido
S. Ignatii ad Paraquar.

Initium emersionis	10	1	0
Digiti obscurati i i	10	6	28
Digiti 6	10	33	2

Eodem

Eodem anno 1729, ibidem *Dec.* 9, post meridiem,

II	3	5	occultavit Luna satellitem	¶	} In eodem oppido S. Ignatii.
II	13	25	præstrinxit Luna limbum	¶	
II	15	0	occultavit Luna totum	¶	

Eclipsis Lunæ observata in oppido S. *Joseph*, anno 1713, *Dec.* 1, p. merid. Differentia meridiana ad *Observ. Reg. Paris.* hor. 3, min. 52, sec. 30.

Initium 10 33 31

Finis 12 56 57

Maxima quantitas obscurata dig. 5, fere ad hor. 11, 45'.

Eclipsis Lunæ observata anno 1717, *Martii* 26, p. merid. in ipso meridiano S. *Cosmæ*. Differentia meridiana a *Paris.* 3^h 52' 20". Sereno et tranquillo cœlo.

Penumbra sensibilis	9	40	0
Initium eclipsis	10	2	21
Digiti obscurati 1	10	8	30
2	10	15	2
3	10	13	41
4	10	31	32
5	10	40	56
6	10	52	8
7	11	10	40

Ope reticuli maxima quantitas obscurata videbatur digitorum 7, min. 18.

Emersis

Emerfio Lunæ ex umbra.

	^h	ⁱ	^{''}
Digiti obfcur. 6	11	45	40
5	12	6	25
4	12	16	35
3	12	24	10
2	12	32	46
1	12	39	25
Finis Eclipfis	12	45	40
Emerfio ex penumbra	13	1	0

Eclipfis Lunæ obfervata in oppido *S. Michaëlis Archangeli* anno 1728, *Februarii* 24 poſt merid. tubo 10 ped.

Differentia temporis inter oppid. *S. Mich.* et Obf. Reg. *Parifinum* 3^h 48' 50".

Initium eclipfis	14	3	35
Finis eclipfis	17	0	37

Digiti obfcurati ad med. ecl. dig. 9. m. 40.

Anno 1700. *Martii* 4 poſt mer. nondum ſacerdotio initiatus obſervavi rudi *Minerva* eclipſim totalem Lunæ in Collegio *Fluentino*, vulgo *de las Corrientes*, cujus differentia meridiana inter *Parifios* eſt 4 2' circiter.

Initium eclip.	13	14
Immerſio totalis	14	34
Initium emerſionis	16	15
Finis eclipſis	17	15

8*

Anno

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Anno 1729, Dec. 21, p. m.

Emerfio Satel. prima. obferv.	^a 7 "	Tubis
in <i>S. Ignat.</i> ad <i>Paraquar.</i>	10 52 49	13 ped.
A clarifs. D. <i>Nic. del' Ifle</i>		
obfervata <i>Petropoli</i>	16 42 36	15 ped.
Diff.	<u>5 49 47</u>	

Anno 1730, Martii 27.

Immerfio Satel. 4 in <i>S. Ign.</i>	7 23 0	18 ped.
<i>Petropoli</i>	13 12 31	13 ped.
Diff.	<u>5 49 31</u>	

Anno 1730, Aprilis 8, p. m.

Emerf. Sat. 2. in <i>S. Ign.</i>	6 36 45	tubo 13 ped.
<i>Petropoli</i>	12 26 15	tubo 13 ped.
Diff. meridiana	<u>5 49 30</u>	

Alia Satellitum Jovis Phænomena, obfervata in op-
pido *S. Ignatii* ad *Paraquariam*, p. m.

Anno
1729, Dec. 29, 14^h 21', fuit conjunctio Primi cum
Secundo: utraque ftella videbatur una.
1730, Jan. 23, 9^h 10', fuit conjunctio Primi et
Secundi.

Jan. 25, 15^h 21' 15". Primus, et Secundus
erant conjuncti, adeo ut uterque videretur
unus. 15^h 27', adhuc vifebatur unus: hora
vero 15, 36' erant difjuncti.

1730,

- 1730, *Mar.* 9, $11^h 36'$, fuit conj^o Sec. et Quarti.
Martii 12, $10^h 9'$, fuit occultatio Secundi
 retrogradi in margine γ .
Martii 18, $6^h 38'$, fuit conjunc. Sec. et Tertii.
Martii 29, $9^h 7' 40''$, fuit occultatio Tertii
 directi in margine γ .
Martii 30, fuit occultatio Secundi Retr. in
 limbo *Jovis*, $7^h 21' 30''$.
Martii 31, fuit occultatio Primi Retr. in
 limbo *Jovis*, $9^h 21' 15''$.
Aprilis 1, $6^h 36' 25''$, tub. ped. 18, obser-
 vavi occultationem Primi Directi in mar-
 gine *Jovis*: hora verò 10, 16' 57" emer-
 sit ex umbra *Jovis*.

- 1729, *Dec.* 9, $11^h 3' 5''$, p. m. præstrinxit margo
 Lunæ Satellitem tertium *Jovis*. Initium
 occultationis *Jovis* fuit $11^h 13' 15''$. Oc-
 cultatio totalis *Jovis* in margine Lunæ
 fuit $11^h 15'$.
 1730, *Aprilis* 27, apparebant *Saturni* anfulæ valde
 exiles; sed *Maii* 3, 17^h , erat *Saturnus* ro-
 tundus, et suis anfulis penitus orbatus.

Observationes astronom. a P. Bonaventura Suarez,
 in Miss. Paraquariæ, Soc. Jesu, in oppido S. Ignatii
 ad Paraquariam. Est aliud oppidum S. Ignatii
 orientalius adumen vulgo *Zabala* nuncupatum.
 Oppidum S. Ignatii (religios occidentalius) distat a
 civitate Assumptionis Paraquariæ versus austrum
 50 leucis Hispanis.

Latitudo Assumptionis a me observata gr. 25, m. 14,
 Austr.

Latitudo S. Ignatii gr. 26, m. 12, I
 Diff.

Diff. mer. S. Ignat.	{ a Petropoli	5	59	40	Omnes occiden.
	{ a Parisiis	3	57	50	
	{ a Londino	3	48	40	
	{ ab opp. S. Cosmae	0	5	30	

Emerfiones Satell. Primi, p. m.

Anno.						
1729	Januarii	26	8	16	42	tubo ped. 13
	Februarii	18	8	28	13	13
	Martii	6	6	50	18	13
	Martii	13	8	47	4	13

Immerfio Primi.

	Novembris	3	16	20	36	13
	Decembris	21	10	52	49	13
1730	Januarii	6	9	0	40	18
	Januarii	13	10	53	8	18

Emerfiones Primi.

1730	Februarii	7	7	46	20	13
	Martii	9	9	56	21	13
	Martii	16	11	53	21	18
	Martii	25	8	49	46	18
	Aprilis	1	10	16	57	18
	Aprilis	17	8	39	55	18
	Maii	3	7	1	47	18
	Maii	10	8	36	51	18

Emerfiones Satell. Secundi, p. m.

1730	Aprilis	8	6	36	45	18
	Maii	10	6	32	30	18

Emerfio Tertii.

1730	Aprilis	20	8	44	45	18
------	---------	----	---	----	----	----

Emerfio Quarti.

1730	Martii	10	9	22	0	dub.
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Immerfio Quarti.

	Martii	27	7	23	0	18
--	--------	----	---	----	---	----

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Forty-fifth VOLUME
O F T H E
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For the YEAR 1748.

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l. ult. for Schenzer read Scheuchzer.
No. 487, p. 320, Tit. VII. for Archdeacon read Dean.
No. 488, for p. 485, read 484, and for 484 read 485.
No. 489, p. 524, and in the Contents, Art. VIII. for Gieschow,
read Grischow.
No. 490, from p. 615 to 655, the Reader is desired to insert the
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